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COTTON GROWING  
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Second Conference  
on  
Cotton Growing  
Problems

JULY 1934

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Report  
and  
Summary of Proceedings

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## INTRODUCTION.

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The Second Conference of Workers on Cotton-Growing Problems was held in London, on July 18th, 19th and 20th, 1934.

The original intention was that the Conference should extend over two days only, and that it should be held at the Corporation's offices, but over 30 papers were received for discussion and the number of those attending reached nearly 100, with the result that a third day had to be given to the Conference, and larger accommodation than the Corporation's own Council room had to be found. The Corporation were fortunate in being able to secure the rooms of the Royal Entomological Society for the meetings, and they desire to put on record at the outset of this Report their appreciation of the valuable assistance and the many courtesies extended to them by the Society and by its President, Dr. S. A. Neave.

### Attendance.

The invitation of the Corporation to send delegates to the Conference was accepted by the Colonial Office and the Board of Trade; the Governments of Kenya, Uganda, Tanganyika Territory, Northern Rhodesia, Nigeria, The Gold Coast, Sierra Leone, Gambia, Cyprus, and The Sudan, and by the Indian Central Cotton Committee and the Imperial College of Tropical Agriculture, Trinidad. The Institutions in this country, members of whose staffs attended, included the Royal Botanic Gardens, Kew; the Universities of Cambridge and Manchester; King's College, London; the Imperial College of Science and Technology; Rothamsted Experimental Station; the Imperial Institute of Entomology; the Imperial Mycological Institute; the Imperial Bureau of Plant Genetics; the British Cotton Industry Research Association; the British Cotton Growing Association; and the Experimental Department of the Fine Cotton Spinners' and Doublers' Association.

The Chairman and Vice-Chairman of the Executive Committee of the Corporation attended, and members of their staffs from the Headquarters office, South Africa, Southern Rhodesia, The Sudan, Nigeria, and the West Indies. Nearly all the members of the Research Station Committee were present on one or other of the days of the Conference, and the Corporation gratefully record their thanks to them for their continued help in connection with their scientific work.

In the following list of those who recorded their attendance, no distinction is made between official delegates and those who were present as visitors :—

|                             |                               |
|-----------------------------|-------------------------------|
| Sir Richard Jackson.        | Sir Harry Lindsay.            |
| Mr. A. Foster.              | Mr. F. K. Jackson.            |
| Sir William Himbury.        | Mr. H. R. Stewart.            |
| Sir James Currie.           | Mr. R. G. Allan.              |
| Dr. J. C. Willis.           | Dr. N. Ahmad.                 |
| Mr. L. G. Killby.           | Mr. K. Sawhney.               |
| Mr. J. C. May.              | Dr. H. D. Nangpal.            |
| Mr. F. A. Stockdale.        | Mr. S. S. Verma.              |
| Mr. W. H. Matthews.         | Mr. R. D. Asama.              |
| Dr. W. L. Balls.            | Professor W. Roberts.         |
| Professor V. H. Blackman.   | Mr. Roger Thomas.             |
| Dr. E. J. Butler.           | Mr. W. O. Sunman.             |
| Professor F. L. Engledow.   | Mr. C. O. Oates.              |
| Professor J. B. S. Haldane. | Dr. J. D. Tothill.            |
| Dr. R. H. Pickard.          | Mr. H. R. Hosking.            |
| Sir John Russell.           | Mr. C. B. Garnett.            |
| Mr. H. C. Sampson.          | Mr. J. G. M. King.            |
| Mr. F. R. Parnell.          | Mr. G. W. Lock.               |
| Mr. F. S. Parsons.          | Mr. C. J. Lewin.              |
| Mr. M. F. Rose.             | Mr. O. J. Voelcker.           |
| Mr. G. S. Cameron.          | Mr. J. West.                  |
| Mr. T. Trought.             | Dr. F. J. Martin.             |
| Dr. A. Skovsted.            | Mr. G. S. Cotterell.          |
| Dr. E. Phillis.             | Mr. G. Cowan.                 |
| Mr. S. H. Evelyn.           | Mr. J. Pirie.                 |
| Mr. A. E. Casement.         | Mr. A. Pitcairn.              |
| Mr. J. Marshall.            | Mr. H. M. Morris.             |
| Mr. G. B. Laurence.         | Mr. M. A. Bailey.             |
| Mr. A. E. Watkins.          | Mr. R. E. Massey.             |
| Mr. F. H. Garner.           | Mr. W. P. L. Cameron.         |
| Dr. H. G. Sanders.          | Mr. E. Mackinnon.             |
| Miss B. Colson.             | Mr. G. F. March.              |
| Dr. F. G. Gregory.          | Mr. A. P. Thompson.           |
| Dr. A. J. Turner.           | Mr. H. E. King.               |
| Miss G. G. Clegg.           | Mr. R. Hewison.               |
| Dr. F. T. Peirce.           | Mr. H. C. Jefferys.           |
| Mr. C. Underwood.           | Sir Geoffrey Evans.           |
| Mr. E. Lord.                | Mr. G. A. Jones.              |
| Dr. B. A. Keen.             | Professor H. R. Briton-Jones. |
| Dr. E. M. Crowther.         | Professor E. E. Cheesman.     |
| Dr. C. B. Williams.         | Mr. G. Gianetti.              |
| Mr. F. Yates.               | Mr. P. E. Turner.             |



Dr. S. A. Neave.  
 Dr. P. S. Hudson.  
 Mr. F. P. Slater.  
 Dr. J. H. Davie.  
 Mr. R. S. Sennitt.

Mr. J. Dawson Shepherd.  
 Mr. L. Vickers-Haviland.  
 Mr. N. Munro Kerr.  
 Mr. N. W. Barritt.

### Procedure.

The procedure was again followed which had proved a marked success at the Conference of 1930. All papers submitted were circulated several weeks in advance, thus enabling those who wished to take part in the discussions to prepare their comments beforehand. At the Conference itself it was thus possible to take all papers as read and their discussion was entered upon immediately after their respective authors had spoken for a few minutes indicating the principal points that their papers were intended to bring forward.

### Programme.

*Wednesday, July 18th.*

- I. The Conference was opened by Sir Richard Jackson, Chairman of the Executive Committee of the Corporation.
  - II. The Corporation invited the views of those attending the Conference as to whether any improvement can be made in the present form of the Annual Reports received from Experiment Stations.
- 
- III. Discussion of papers :—

#### PLANT BREEDING AND GENETICS.

(*Chairman* : Professor J. B. S. HALDANE, F.R.S.).

1. The practical application of genetical science to plant breeding (A. E. Watkins—*Cambridge University, School of Agriculture*).
2. The value of inter-specific hybrids in cotton from the standpoint of genetics (Note received from S. C. Harland—*Cotton Research Station, Trinidad*).
3. Selection effects in pure lines of Sea Island cotton self-fertilized for seventeen generations (Note received from S. C. Harland—*Cotton Research Station, Trinidad*).
4. Cyto-genetics in relation to plant breeding in cotton (A. Skovsted—*Cotton Research Station, Trinidad*).

5. Preliminary testing of new varieties or types of cotton (T. Trought—*The Sudan*).
6. Cotton breeding problems in Northern Nigeria (*Department of Agriculture, Nigeria*).
7. The cotton problems of Kenya (*Department of Agriculture, Kenya*).

#### CROP EXPERIMENTATION AND STATISTICAL TREATMENT.

(Chairman : Professor F. L. ENGLEADOW.)

8. Purity Targets (Note received from C. G. Hansford and J. D. Jameson—*Uganda*).
9. The observation, measurement and recording of crop loss and crop yield (E.C.G.C. staff—*Southern Rhodesia*).
10. Analysis of variance and analysis of co-variance, their meaning and their application in crop experimentation (Note received from J. Wishart—*Cambridge University, School of Agriculture*).
11. Sampling and growth observations in plant development studies on cotton (Note received from O. V. S. Heath—*Barberton*).

*Thursday, July 19th.*

#### COTTON PESTS.

(Chairman : Dr. S. A. NEAVE.)

12. Field studies on the relation of insect pests to climatic conditions, with special reference to cotton (C. B. Williams—*Rothamsted*).
13. Climatic and soil factors in relation to prevalence of, and damage by, insects ; possibilities of reduction of loss of crop due to insects, by the selection and restriction of sowing periods and by the propagation of cotton strains better adapted to local conditions (Note received from H. Hargreaves—*Uganda*).
14. Investigations on cotton insect pests (E.C.G.C. staff—*Barberton*).
15. Interrelationship of wild host plants and cotton, with reference to seasonal variation of stainer population (Note received from A. G. Bebbington and W. Allan—*Northern Rhodesia*).
16. Problems connected with the control of Pink Bollworm in the Sudan (Note received from H. W. Bedford—*The Sudan*).

#### COTTON DISEASES.

(Chairman : Dr. E. J. BUTLER, F.R.S.).

17. Angular Leaf Spot and Blackarm disease (R. E. Massey—*The Sudan*).

18. Blackarm disease in Uganda (Note received from C. G. Hansford—*Uganda*).
19. A Leaf Curl disease of cotton in the Fiji Islands (Note received from R. R. Anson—*Fiji*).

#### BOTANICAL PROBLEMS.

(*Chairman* : Professor V. H. BLACKMAN, F.R.S.).

20. The health and vigour of the cotton plant in relation to its environment (F. K. Jackson—*Institute of Plant Industry, Indore*).
21. Some outstanding physiological problems in the culture of cotton in the Sudan (F. G. Gregory—*Imperial College of Science and Technology*).

*Friday, July 20th.*

#### FIBRE PROPERTIES OF COTTON.

(*Chairman* : Dr. R. H. PICKARD, F.R.S.).

22. Studies in variability of cotton with special reference to immaturity (F. T. Peirce and E. Lord—*Shirley Institute*).
23. The most troublesome impurities in cotton ; bearded motes and neps (G. G. Clegg—*Shirley Institute*).
24. General review of spinning tests on Empire cottons (C. Underwood—*Shirley Institute*).
25. The relation between fibre properties and spinning value (A. J. Turner and C. Underwood—*Shirley Institute*).
26. Brokers' reports on cotton (H. R. Hosking—*Uganda*).
27. The effect of type of gin on the spinning properties (and hence value) of Uganda cotton (H. R. Hosking—*Uganda*).
28. The Tobler Bale Sampler for lint grading purposes (H. R. Hosking—*Uganda*).
29. Moisture determination in bales by radio technique (Progress report by Dr. W. L. Balls).

#### SOIL PROBLEMS.

(*Chairman* : Sir JOHN RUSSELL, F.R.S.).

30. Physical measurement of soil in relation to type and fertility (B. A. Keen—*Rothamsted*).
31. Soil organic matter and crop rotation (E. M. Crowther—*Rothamsted*).

## PRELIMINARY PROCEEDINGS.

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*Chairman :* SIR RICHARD JACKSON.

In opening the Conference, the Chairman expressed the regret of Lord Derby, the President of the Council of the Corporation, at his inability to be present owing to the visit of His Majesty the King, to Lancashire.

Sir Richard said that the gratifying fact that the number of those attending the Conference was nearly double that of those present at the first Conference of this kind held four years ago, was an indication that the former Conference had been a success, and that those engaged on work connected with cotton-growing were glad of the opportunities thus afforded of exchanging views on their common problems.

He welcomed the members of the Corporation's Research Committee who were nearly all present, and thanked them for the very great help that they generously gave the Corporation on the scientific side of their work. He said he felt sure that if any of those present cared in future to ask the advice of any individual member of that Committee, whose acquaintance they made at the Conference, on points connected with their work, they would meet with as ready a response as did the many calls already made on the Committee by the Corporation.

He welcomed several scientific members of the Indian Central Cotton Committee, whose presence afforded a further opportunity of cementing the cordial relationship that had always existed between that body and the Corporation. He was also glad to see several other workers on cotton problems in India.

He expressed his pleasure that those accepting the Corporation's invitation to attend the Conference had included the Colonial Office, the Board of Trade, the Director of Agriculture, Uganda, and the Controller of Agricultural Research in the Sudan.

In conclusion the Chairman conveyed the warm thanks of the Corporation to Mr. Stockdale and Mr. Hibbert of the Colonial Office, for the great help they had given in the work of organizing the Conference.

At the request of the Corporation, the Conference first considered the following note on the form of the Annual Reports from Experiment Stations.

### REPORTS FROM EXPERIMENT STATIONS.

The Corporation wish to take the exceptional opportunity afforded by this Conference to ascertain the view of workers on cotton-growing



## REPORTS FROM EXPERIMENT STATIONS.

problems on the form that the annual Reports received from Experiment Stations should take.

The difficulty encountered is common to reports of all applied research work, and put briefly is this :—How are the results of such work to be presented, bearing in mind the dual purpose that they are required to serve ? Thus (*a*) they must convey a clear idea of the objects and results of the work to those who probably have little or no technical knowledge of the kind of work described, but to whom the results are of sufficient importance for them to have financed it, and who are therefore entitled to have those results put before them in a form in which they can understand them.

(*b*) They must be directly serviceable to those engaged in similar work in places other than that in which the original experiments were carried out—thus saving time and labour and unnecessary repetition.

The Corporation have now issued their volume of Reports from Experiment Stations annually since 1923. After two years it was clear that the subject matter was both too technical and too detailed for any readers but workers on the same or allied subjects, and the Executive Committee were fortunate in enlisting the services of Dr. Willis to write a preface to the volume, in which the contents of the reports were reviewed in such a way as to bring out for the benefit of the general reader the main lines that the various investigations were following and the principal results obtained. But as the Corporation's work extended, the number of Experiment Stations increased and the volume of reports grew unwieldy. The Research Committee discussed the question, and on their recommendation the several Stations were asked their views on a proposal that each Station should produce a full report every third year only. With twelve Stations, each annual volume would thus contain four full reports. Any Station should be at liberty, however, to include between its full reports an account of any piece of work that was nearing, or had just reached, completion. In the years that intervened between the full reports, each Station should submit only a short general description, in non-technical language as far as possible, of the work of the past season, including also such tables and graphs as might be needed to illustrate the principal results, and to preserve the continuity of such records. All Stations should, further, include in these summarised reports, or in the form of a separate note if desired, any observations or results that might be of immediate use to co-workers.

It was hoped that this scheme, in conjunction with Dr. Willis' general review as preface, would go some way towards producing a volume of reports that would fulfil both the purposes described

above. The procedure has, however, only been partially successful; the authors of some of the summarised reports have evidently found a difficulty in producing the "short general description in non-technical language, as far as possible, of the work of the past season"—admittedly no easy matter, even to those who have had long experience.

The question arises, therefore, as to how an improvement can be made, keeping in view the necessity for producing an annual Report that will give a clear and concise, but adequate idea of the progress made and the difficulties encountered, and also for including sufficient detail to enable workers on similar problems to gauge the value of the work and make use of its results, thus expediting progress.

One suggestion that has been made is that (i) the annual Reports from all Stations should consist every year of a general account only of the problems investigated and the results obtained (with the proviso mentioned above that everything believed to be of immediate help to fellow workers might be included, in such detail as is thought necessary, in the form of a separate note); (ii) the separate pieces of work should be submitted for publication as soon as they were completed, under the names of the respective authors, to the editors of appropriate scientific journals; the Corporation might then purchase reprints and distribute them to other members of their staff, and possibly to workers on similar problems on the staffs of Agricultural Departments in cotton-growing countries where they had no staff of their own.

The Corporation desire to take advantage of this Conference to hear the views of those present on this proposal, and to receive alternative suggestions as to what the best form for these Reports to take with a view to (a) keeping the cotton industry in touch with the Corporation's experimental work and maintaining their interest, and (b) disseminating information that may be useful at other Stations, bearing in mind the finding of the Milner Committee that "Money spent on agricultural investigation and research must, it seems to us, be largely wasted if the results obtained are not fully recorded and made available to all."

#### DISCUSSION.

MR. PARNELL said that the Note that had been circulated brought out the difficulties that confronted the authors of these Reports. Provision must undoubtedly continue to be made for two different categories of readers, and after consultation with his colleagues at the Barberton Station, he had come to the conclusion that it would be best to publish

## REPORTS FROM EXPERIMENT STATIONS.

*the more definitely scientific work in appropriate scientific journals, and to issue a separate report in non-technical language for distribution to members of the Corporation and to the general public. Even if this were done, however, the difficulty would not have been fully met. All workers on cotton, whether scientific specialists in Agricultural Departments or District Agricultural Officers in cotton-growing countries ought to read the collected reports of experiment stations; they would not by any means all read the scientific journals, and what was needed for them was a Report in a form that could be understood without any special effort in the sort of climate in which the majority of them had to work.*

*He raised the question whether, if such reports were published, scientific journals might decline to accept papers dealing in detail with the more scientific aspects of the work described in the Reports, on the ground that the main portion of the subject-matter had appeared elsewhere.*

*He thought that particularly the younger members of the Corporation's staff would probably be glad to publish their results and get the recognition of their work that such publication might bring. He put forward tentatively a proposal that the Corporation might publish (i) a small volume for their own members and the general public consisting practically of a collection of the summaries that already appear at the beginning of each Report, but probably somewhat amplified, and (ii) a longer report intended for agricultural officers and scientific workers generally which should be readily understandable; and finally that those pieces of work that are worth it should be sent to appropriate scientific journals for publication.*

PROFESSOR ENGLEADOW said that speaking only as one who reads the reports, but did not have to write them, he thought that a report suited to lay readers, such as members of the Corporation, was necessary, but that it should be a separate publication. It was too much to expect scientific and agricultural workers to prepare one report that would be at the same time of interest and assistance to their colleagues and intelligible to the lay reader.

*The utmost use should be made of appropriate scientific journals, and he saw no reason to believe that they would be unwilling to accept a paper because part of its substance was contained in the Corporation's Reports, where it certainly would not appear as a scientific record of a finished piece of work.*

*The third question was what should constitute the Station Report itself. Eliminating that part which aimed at keeping lay readers—and especially members of the Corporation—in touch with what the Experiment Stations were doing, and leaving out also finished pieces of scientific work, which would appear in due course in scientific journals, one was left with what might be called a progress report. Such a*

report should be so framed as to achieve two objects : firstly, it should be a progress report, as he had just said ; and secondly, it should be a useful report for others working on similar subjects.

He suggested that in framing such a report, brevity was absolutely essential. There were many Stations, and often several workers at each Station, and it was necessary that the volume should not become bulky and unwieldy. He believed that one of the best means of attaining the desired brevity was to rule out definitely the notebook element in the reports ; in other words, save where it was essential, neither the experimental data nor the statistical computations should be given in full. In general, only the final statistical result of the test of significance need appear, and none of its arithmetic. For the rest, brevity should be aimed at throughout.

MR. TROUGHT agreed with previous speakers as to the need for a summarised report, but said that a fairly detailed report was extremely useful to the authors themselves, as such a report enabled records of results of previous seasons to be readily found, without necessitating reference to old files. In addition, he advocated the issue by each Station every three or four years of a summary in quite simple language of the progress made and the main results achieved during the period covered. Such a survey, he thought, would be useful to other stations doing similar work.

## PAPERS AND DISCUSSIONS.

### PLANT BREEDING AND GENETICS.

(Chairman : Professor J. B. S. HALDANE, F.R.S.)

#### THE PRACTICAL APPLICATION OF GENETICAL SCIENCE TO PLANT BREEDING.

BY

A. E. WATKINS (*School of Agriculture, Cambridge*).

Mendelian methods have now been applied to plant breeding for 25 years or more ; and with this experience it should be possible to form some idea of the extent to which genetical science has assisted the plant breeder in the past, and to judge what contributions it may make in the near future. In attempting this it must be realised at once that in past ages outstanding individuals have had remarkable success in improving plants. The pure line principle was adopted as a basis for wheat breeding by Colonel Le Couteur some 80 years before it was discovered by science ; and in 1873



## THE APPLICATION OF GENETICS TO PLANT BREEDING.

Shirreff described a method of cereal breeding that was not far removed from modern practice: "The object in all probability will be sooner accomplished and better controlled, by first creating a diversity, which can easily be effected by crossing. . . . Always cross with the seedlings which inherit in the greatest degree the properties you wish a cereal to possess, and by persevering for a series of years to select, and by crossing in this manner, success in all probability will be ultimately attained." Between them these two workers discovered the most important principles. The only big advantage we have over them to-day is that knowing something of heterozygotes and homozygotes we are likely to find out much more quickly than they what will breed true and what will not. But it is possible that the most valuable uses of elementary genetics are, first, that it keeps us from making fundamental mistakes—we should not to-day spend most of our lives on selection within a pure line, as Major Hallet did—and, secondly, that instead of our having to wait for the occasional genius, it is possible for workers to be trained at any time to a standard of efficiency which may be greater than that of any past exponent of the art.

It remains true, however, that the two principles of pure lines and character re-combination were used by practical breeders long before they were discovered by science. We may be able to apply these principles more readily now, but so far no new principle has been added: and this makes it useful to enquire how far we are likely to improve upon earlier workers, and whether there are any more recent genetical discoveries that may help us in our task.

In many cases—Marquis wheat is an obvious example—the productions of pre-Mendelian times reached a very high standard, and improvement over these achievements may prove to be difficult. The position in England is instructive. In the first place, the improvements of the last century brought cereal varieties to a high level of excellence. A further advance has since been made—in the case of wheat by the addition of the characters good quality and rust resistance—through the application of genetical knowledge. Viewing the matter broadly to-day, it can be said, I think, that we have in this country varieties of wheat and barley in which most of the characters we want are developed to the maximum at present known in their respective genera. Further improvement is theoretically possible, but will be difficult: it might reach an appreciable value if spread over the whole country, but would not be spectacular. Rapid improvement would only be likely if

a demand sprang up for a new type of variety altogether. If the farmer wished, in view of the cheapness of artificial manures, to grow heavily manured crops that would stand up by virtue of a very short straw, a series of satisfactory varieties could probably be produced in a short time. In fact, it is with the rise of new conditions, or in newly developing countries, that the Mendelian method is most likely to give quick successes. In these circumstances the value of the achievements may be very great; but in less favourable conditions over confidence is not likely to be an asset. This probably applies also to less advanced areas than Western Europe; it seems possible that in Iraq, for example, the improvement of date palms would be no easy matter.

As usually practised to-day, plant breeding involves two separate processes. The first is the finding of varieties in which the useful characters reach their maximum development; and the second is the combination of all these useful characters in a single individual, by the appropriate crosses. To attain the first object the accepted method is to collect as many varieties as possible and try them. This has been rendered easier by Vavilov's discoveries, and nowadays it should be possible to obtain a comprehensive collection of the varieties of many of our cultivated plants quite easily. In the case of cotton it seems doubtful whether an adequate collection is yet available, outside Russia. This comprehensive collection is one of the advantages we have over the older breeders; but it should not be relied on too much, especially in the case of countries in which a crop is long established, since in these circumstances local varieties are generally highly adapted to their special conditions. Until recently, collecting was the only possible method of trying to find what was wanted. To-day there are indications that it may be possible to increase the range of available variation artificially. So far there are two known ways of producing new variants: by X-raying; and by the formation of new polyploid forms, either by hybridisation or in some other way. The first of these does not seem to me, at present, to be very promising from the economic point of view; though it is clearly important that the method should be properly investigated. On the other hand the production of new polyploids seems to me very important. In the first place it breaks fresh ground, and suggests the possibility of making entirely new species of cultivated plants. Secondly, polyploidy itself may, in some cases, be responsible for increased size. And finally, a doubling in chromosome number makes it possible to conserve hybrid vigour. Moreover, in one case it has already proved its importance in

practice, for the new Javan sugar canes, which are said to be outstandingly successful, are new polyploids. Personally I regard this method as one which it is of the utmost importance to investigate; but there are several technical difficulties, and success will probably depend more than anything else upon the selection of the right genera. It is a decidedly speculative method: but if successful, as it is almost certain to be sooner or later in some plant or other, the improvement obtained may be revolutionary in character. Whether it is ever likely to be of any use in cotton cannot be said, since the knowledge necessary to form a judgment is not, so far as I know, available.

After finding the most suitable variants the next task of the plant breeder is to combine them by crossing. This is a much more difficult matter to discuss, since the technical methods necessary are so different in different plants. So far, it is only in self-fertilised plants that scientific principles have been applied. In such cases the methods used are familiar and, as I have pointed out, the principles of to-day were practised by a few workers in pre-Mendelian times, though they can be applied now more easily and precisely. The only new prospect that has been opened out is the crossing of widely separated species instead of nearly related varieties. This appears to have had some success in America, where rust resistant *vulgare* wheats have been obtained by crossing with Emmer. In general, wide crosses are not to be recommended lightly, since they introduce many difficulties; but they could probably be used with more chance of success to-day than in the past, since the genetical principles involved have now been worked out.

Before leaving self-fertilised plants I should like to draw attention to one way in which the plant breeder could give the genetical worker valuable help. Within the last few years, as one approach to the problem of evolution, the geneticist has begun to think about applying his knowledge to natural populations. In this direction the plant breeder should have had considerable experience, since he watches the multiplication of varieties for several generations on a large scale, under controlled conditions. With a plant like cotton a variety could change its character in course of time as the result of several known causes. In the first place the original strain might have been heterozygous for certain factors. This would be especially important if the different segregates multiplied at different rates. Secondly, natural crossing might occur: though this should, of course, be prevented if possible. And, finally, new mutants could arise, their possible importance depending partly upon mutation rate, and partly upon

the rate at which they reproduced themselves—whether more slowly or more quickly than the old type and how much so. Can the plant breeder give any exact information about the importance of these different factors in any given case?

In crops that are normally cross-pollinated and reproduced by seed we have a very different situation. There are considerable technical difficulties in the way of the breeder and no really satisfactory method of breeding has been devised. All that can usually be done is to practise some sort of selection. In the sugar beet, this was highly successful during the greater part of the nineteenth century, but little progress has been made since then, and it looks as if the limit is being approached. There is no doubt that in such crops selection will do astonishing things at first. It may even make an unimportant character important. Working in America, Winter reports that the oil content of maize seeds has been raised from  $4\frac{1}{2}$  per cent. to 10 per cent. in 28 years, is still increasing, and shows no sign of having yet reached the limit. Of course this has been possible because nobody has ever wanted to select for oil in maize. Selection for yield has had very different results. Indeed it is hardly too much to say that, with one exception, no sensible improvement has been effected in the yield of maize in America by the application of scientific methods to breeding. The exception is the exploitation of hybrid vigour, which appears to have had some measure of success. Probably an investigation of hybrid vigour would be the most hopeful way of helping the breeder of this kind of plant; and, certainly, not only does hybrid vigour seem to be of fundamental importance to cross-fertilised plants, but it is also difficult to suggest any other line of attack. Whether hybrid vigour occurs in cotton I do not know; but even if it does it could only be conserved by doubling the chromosome number, and even then it is doubtful whether the plants so obtained would breed true.

The last class of plant to mention is that of cross-fertilised plants which are vegetatively propagated, such as the sugar cane and most fruits. The individuals that form varieties are usually very complex genetically; often with irregular chromosome behaviour and partially sterile. Breeding behaviour is likely to be complicated; and *a priori* it would be expected that recent advances in genetical knowledge would greatly help the plant breeder to bring about improvement in some cases. The cotton worker is not likely to be concerned unless cotton could ever be reproduced vegetatively at a cost that is economic.



## THE APPLICATION OF GENETICS TO PLANT BREEDING.

It may, I think, be concluded that in new countries, or to meet a change in demand, the skilled worker may produce important results by the application of Mendelism and the pure line principle to self-fertilised plants. After a time, and in countries with a highly developed agriculture, improvement becomes increasingly difficult. In cross-fertilised plants that are reproduced by seed scientific breeding has not made any recent advance, except probably for one case in maize; but in those that are reproduced vegetatively there may be a hopeful field. There is a distinct likelihood that recent genetical discoveries, or discoveries yet to be made, may result in great improvements; but at present this seems to me likely to come only in certain selected genera. Which these genera are to be it would be too hazardous to predict.

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Mr. WATKINS, in his introductory remarks, said that he had two objects in writing this paper. The first was to discuss whether the genetical worker had anything to offer the plant breeder; the second, to discuss the principal factors which seemed likely to determine, in an average case, whether the plant breeder was likely to be successful or not.

The importance of these considerations was obvious. For instance, in the report of the previous Conference of this kind, it was suggested that a territory that was as yet undeveloped could not afford a plant breeder. If what he had said in his paper were accepted as correct, it was precisely in such a case that the employment of a plant breeder was likely to give the most striking results.

The comments made by Dr. Harland, in a Note which had been circulated, appeared to him to support very largely what he had written in his paper. Dr. Harland's criticisms were briefly these. First, he suggested that selection in reputedly pure lines might be advantageous in certain circumstances, and that was in accordance with the speaker's expectations. Evidently cotton was a plant in which purity was extremely difficult to obtain, and that was naturally one of the breeder's first objectives. Secondly, Dr. Harland referred to the technique of the repeated back-cross in breeding. That, he thought, was merely a question of the technique adopted to attain the objects discussed in the paper.

The only question he would like to raise was in connection with breeding cross-fertilised plants, where Dr. Harland thought that more advances had been made in the technique of breeding than was indicated. He would like to ask whether any further information

could be given on that matter, because his own impression was that the technique had not got very far. He asked whether it had been found possible, as a result of the methods to which he alluded, to put on the market new varieties which were a great advance on the old.

Finally, if the views put forward in the paper were correct, he had certain expectations about cotton breeding, and he would like to know whether those expectations had been fulfilled.

First, cotton evidently became mixed very easily, so that he would think one of the main achievements of the plant breeder would have been to produce much purer types than existed formerly. Secondly, there had very possibly been changes in the demands of manufacturers with regard to quality. If that were the case, he would expect that the plant breeder had had success in meeting those demands. Finally, to some extent cotton had been introduced in places where it had not been grown before, and he would expect that the plant breeder had been successful in producing varieties suited to those new places.

Those were the principal directions in which he would have expected the plant breeder to have had his main successes. He would be very glad to learn whether his impressions—and therefore the general principles communicated in the paper—were sound.

#### DISCUSSION.

DR. BALLS said that after thirty years of more or less uninterrupted contact with cotton-breeding problems, he thought that Mr. Watkins was unduly pessimistic as to the possibilities of the more rigid interpretation of breeding work which had been rendered possible by Mendel's law and the work of Morgan. Le Couteur, Shirreff and Hallet were, of course, doing exactly what plant breeders were doing to-day—self-fertilising, selecting, and back-crossing—but, speaking as one of the first three people to attempt to apply Mendel's law to the cotton plant, his recollection of that early work was that it was like directing a torchlight into a cellar. Workers felt that though they were still fumbling, and were still in gloom, at any rate there was a star to guide them, and that same difference in outlook still persisted.

He had said years ago that the definition of a pure line was a definition in a vicious circle; a pure line was a thing in which no difference between successive generations of offspring could be found, but it did not follow that the differences were not there. He gave two instances that had occurred in Egypt in recent years of the effect of applying a new discriminating test to lines which were pure according to all the tests which they had been able to apply up to that time. The first case was a test for resistance to wilt. Certain lines as to the purity of which they were quite satisfied by all the characters that could be used for discrimination, when tested for resistance to wilt had

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shown themselves to be hybrid, and some interesting wilt resistance phenomena had been tracked down. About four years ago further tests had been carried out discriminating between different plants and families on the basis of the properties of the cotton hair, and again it had been found that one or two lines showed purity, while others segregated.

Le Couteur, Hallett and Shirreff would have had to treat those things as simple selection properties, but it was now possible to trace the underlying mechanism by which such segregations occurred, and so workers were enabled to provide themselves with definite reasons for conducting certain operations in the hope of achieving a practical result, and actually achieving it.

Dr. Balls mentioned further that in 1909 he had made a cross between two strains, 77 and 310, and in the fourth generation a family turned up with a most unusual habit of growth. It was illustrated in his book "*Raw Cotton*." There was one complete  $F_4$  family, and there was one plant in another  $F_4$  all by itself. The inference from this double occurrence was that it was a recombination of factors; genes had met, and the product was a definite segregate. At that stage, in 1913, he had left Egypt; the strain was not carried on, and eventually all the seed died. On his return to Egypt in 1928 he had suggested to his geneticist that he should find the stocks of the old seed and repeat the cross. He did so, but Dr. Balls had laid the matter aside. Just before he came home this year he was shown by his colleagues a most strange field of cotton; a quarter of an acre of the same thing which had turned up again. Thus genes and pure lines were realities, and after twenty years it was possible to resurrect the thing which you had lost in the past.

MR. EVELYN supported Dr. Harland's statement regarding the advantages which repeated back-crossing had over the old method of growing  $F_2$ s and  $F_3$ s. The experience at the St. Vincent Station was that the method of repeated back-crossing was easier and quicker when performing crosses in which it was intended to transfer certain genes from one variety to another. As an example, he quoted the experiment that had been made with the object of producing a Superfine Sea Island plant with a red plant body: a cross was made between Trinidad Red Kidney—a red perennial Peruvian type—and Superfine St. Vincent (V.135). By back-crossing the heterozygote to the Sea Island parent three times it had been possible to select a plant breeding true to red plant body with lint length longer if anything than the V.135 parent, i.e., 60 mm. He doubted if this result could have been obtained so easily and quickly by growing large  $F_2$ s and  $F_3$ s, particularly as in this case it was almost impossible to differentiate between dominant homozygotes and heterozygotes.

Mr. Evelyn said he would welcome any further information on the method of repeated back-crossing as in all cases of gene transference at the St. Vincent Station that method was being

employed in preference to the old  $F_2$  and  $F_3$  method which they had now almost completely abandoned.

MR. JACKSON thought that Mr. Watkins took too gloomy a view of the difficulties that confronted the plant breeder when attempting selection work in a country in which cotton had been grown for a number of years. At Indore no difficulty had been experienced in making considerable improvements by that means in the cotton crops of Central India. In any field of cotton as grown by the ordinary cultivator great differences in lint length and lint quality still showed themselves, and selection should produce definite improvements.

MR. TROUGHT suggested that one of the difficulties of the back-cross technique was that the so-called pure line parent might not really be pure, so that having got the  $F_1$ , the progeny of the so-called pure line parent might not be of the same type, with the result that the back-cross would be on to a different type; it was consequently eventually necessary to revert to selection in those progenies.

PROFESSOR CHEESMAN said that in tropical crops other than cotton (of which he had had no breeding experience) the reason why genetic principles had not been applied on a much wider scale was that there was still such a wide field open to simple selection with a view to purification before further advances were economically feasible.

DR. HUDSON agreed with the previous speaker as to the scope that still remained open to straightforward selection. Mr. Jackson's remarks also supported this view, nor did he think that there was necessarily any discrepancy between what Mr. Jackson had said and Mr. Watkins' statement that it was in a new country that the most striking results could be expected from plant breeding if it could be assumed that Mr. Watkins referred to a country in which organised plant breeding had not hitherto been carried out, not merely to a country in which the crop had not hitherto been grown at all. In reply to Mr. Watkins' question as to the introduction by plant breeding of new varieties of plants, certain new and extremely profitable strains of maize had been introduced by these means.

MR. WATKINS, replying, said that he had not intended his remarks to be pessimistic, but he would prefer to call them "guarded." The statements in his paper had been carefully qualified, and he was glad to learn that in the case of cotton, plant breeders evidently had a fine field before them.

## THE VALUE OF INTER-SPECIFIC HYBRIDS IN COTTON FROM THE STANDPOINT OF GENETICS.

BY

S. C. HARLAND (Cotton Research Station, Trinidad).

The opinion has often been expressed by cotton breeders that interspecific hybrids within the genus *Gossypium* are of little or no value from a practical standpoint. On the other hand, it is claimed by Russian



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workers that interspecific hybrids, particularly between Upland and Egyptian cotton, offer great possibilities.

The writer while on a visit to Turkestan saw experiments with Upland-Egyptian hybrids carried out on a vast scale, but was obliged to come to the general conclusion that even the best of the synthetic types was seldom as good as either parent, and none was demonstrably better.

O. F. Cook in a bulletin published some years ago was decidedly against American-Egyptian hybrids from a practical standpoint.

The long-staple Upland variety Meade is said to be the outcome of a cross between Upland and Sea Island and this diluted by Upland genes. Experimental work indicates that Meade is not a typical Upland cotton and the story of its origin is possibly true.

Part of the difficulty felt by the cotton breeder lies in the fact that the taxonomy of the genus *Gossypium* has only recently been placed on anything like a scientific footing, and there were no experimental facts to indicate the degree of relationship between the various species, either genetically or cytologically. Recently, however, genetical and cytological studies at this laboratory provided criteria by means of which the cotton breeder may obtain some guidance in the difficult task of improving cotton by interspecific hybridisation.

Dr. Skovsted, in a paper presented to this Conference has put forward his conclusions respecting the relationship of the wild 13-chromosome species to the 26-chromosome cultivated cottons of the New World. He states that from a cytological point of view there are no obstacles to the incorporation of certain valuable economic properties of the wild species into the New World genotype. Cytological and genetical considerations, however, do not always agree and it may be expedient to discuss the whole problem of the transference of characters from one species to another, more particularly from a genetical standpoint.

Without going into the taxonomy of *Gossypium* in great detail it may be said that the problem of transference is not identical in all interspecies crosses. As has been previously shown, the genus *Gossypium* is divided into species with 13 chromosomes and species with 26 chromosomes. All the cultivated Asiatics have 13 chromosomes and the relationship between the *Arboreum* group and the *Herbaceum* group appears to be rather close, so that species hybrids in the cultivated Asiatics are not confronted with serious obstacles in the way of sterility or incompatibility. The only 13-chromosome species which at present offers any possibilities for combination with Asiatic types

is the species *Gossypium anomalum*, which grows wild in Somaliland and other parts of Africa. Hybrids involving *anomalum* were successfully made by J. B. Hutchinson, but have not been sufficiently studied to enable their value to be stated.

The problem of improvement of Asiatic cottons seems to lie in the full utilisation by synthesis of economic characters scattered throughout the *herbaceum* and *arboreum* groups. It seems to be unnecessary to go outside these groups, although the possibilities of artificial tetraploidisation should be fully exploited.

When we come to the New World 26-chromosome group we are confronted with a series of 26-chromosome types having the rank of true species. These are as follows :—

- (1) *G. barbadense*, L. (Egyptian, Upland, Ishan, Kidney, Peruvian, Tanguis, etc.).
- (2) *G. hirsutum*, L. (Upland).
- (3) *G. purpurascens*, Poir (Perennial Bourbon).
- (4) *G. purpurascens*, var *punctatum* (annual or semi-annual small-petalled Bourbon).
- (5) *G. tomentosum*, Nutt (The wild cotton of Hawaii).
- (6) *G. Darwinii* (The wild cotton of the Galapagos Islands).
- (7) *G. taitense*, Parl (The wild cotton of Polynesia).

The taxonomic relationships within the New World group are yielding to genetic analysis. The situation is somewhat as follows :—*Barbadense* and *Darwinii* are taxonomically somewhat alike. *Hirsutum*, *purpurascens* and *taitense* are closely allied. The position of *tomentosum* is somewhat obscure, but it possesses evidence of both *hirsutum* and *barbadense*, with possibly a rather closer relationship to *barbadense*. It has been shown at this laboratory that hybrids within all these species are easily effected and that although some sterility occasionally occurs in the  $F_2$  of such hybrids, complete fertility and recombination of characters from both species can take place in  $F_3$  and subsequent generations.

Although fertile combinations of any two New World species are possible, this is not the same thing as saying that the resulting combinations are of any value from the economic point of view. It is quite easy to say that *Gossypium tomentosum* is a xerophytic type habituated to an arid environment and poor soil, and another thing to say that this valuable character can be incorporated into Upland or Sea Island cotton. The problem of transferring drought resistance

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from *Tomentosum* to Upland would be a task which a few years ago would be embarked on by the plant breeder in a lighthearted spirit of optimism. Nowadays we are by no means optimistic about the outcome of such experiments, and we realise that much fundamental work in genetics has to be done before the possibilities of practical plant breeding can be stated. The ordinary method of plant breeding is to cross two species, grow a more or less big  $F_2$ , spend a lot of time in making frequency arrays of measurable characters which refuse to behave in rational Mendelian manner, grow a lot of  $F_3$  and  $F_4$  families with the hope of finding desirable combinations, and then either abandon the work or put something into cultivation which proves inferior after extensive practical tests. It is thought that by obtaining millions of plants in  $F_2$  something valuable will turn up. So far as I know, the practical results of such work are very meagre, and only in special cases where the species have been apparently closely related has any real advance been made.

Genetics has in the last few years gone very far beyond ordinary plant breeding technique, and the latter has failed singularly to profit by new fundamental principles which are the outcome of modern studies of genetics.

Some definite information is available respecting genetic relationships in Upland and Sea Island-Egyptian. These two species almost certainly have their centres of origin in the southern part of Mexico and in Ecuador or Peru respectively. They have thus been isolated for a long period of time—probably for some millions of years—and their characters have come to be genetically constructed in quite different ways. It is not established with certainty whether these two species possess any genes in common at all. It is likely that any given gene in one species is represented by a characteristic "species allelomorph" in the other species.

Both Upland and Egyptian each possess a co-ordinated genic system adapted to a given set of environmental conditions. In the  $F_2$  of a cross between them there is mutual disintegration of genic systems and a series of maladjusted and disharmonic combinations result. A useful analogy is to imagine that two motor cars of different makes are taken to pieces and an attempt made to produce with the pieces a new type of car with the valuable features of both. This is clearly impossible. Some parts could be interchanged, but important structural units could not be combined. This, it is believed, is the situation in crosses as wide apart as Upland  $\times$  Egyptian or Sea Island  $\times$  *purpurascens*. It has been shewn that single genes can be transferred from one species to the other by repeatedly back crossing. Four to five back-crosses are usually necessary for such transference.

At the present time it is idle to indulge in speculation about the value of interspecific crosses in plant breeding. It is urgently necessary to have exact experimental data on the following points :—

1. To what extent can single genes be transferred from one species to another ?

If for example the genes *y* (cream corolla), *p* (cream pollen) and *s* (no spot) are transferred from Upland to Sea Island, are there any effects on lint length or lint quality. This particular experiment has now been carried out and a positive answer to the question will be forthcoming next season.

The following are a few of the transferences which have been effected in interspecies crosses :—

| <i>Character.</i> | <i>Transferred from</i>                | <i>Remarks on New Type.</i>                |
|-------------------|----------------------------------------|--------------------------------------------|
| Hairiness         | <i>tomentosum</i> to <i>barbadense</i> | Intensely hairy (longish hairs).           |
| „                 | „ to <i>hirsutum</i>                   | Very hairy (short hairs).                  |
| White lint        | Sea Island to Egyptian                 | Pale brown with longer lint than Egyptian. |
| Brown lint        | Egyptian to <i>hirsutum</i>            | Very pale brown lint.                      |
| Lacinated leaf    | Upland to Sea Island                   | More lacinated and weak.                   |
| Long lint         | Sea Island to Upland                   | Lint length reduced to circa 41 mm.        |
| Naked Seed        | Upland to Sea Island                   | Practically lintless.                      |
| Red leaf          | Upland to Sea Island                   | Red much weakened.                         |
| Crinkled          | Sea Island to Upland                   | Less vigorous and productive.              |
| Red leaf          | Kidney tree to Sea Island              | Little change.                             |

These transferences are only a few of those which have been attempted, but it seems that certain single gene transferences may be of some economic value. The new hairy Sea Island for example should be valuable in some localities, and this character could easily be transferred to Egyptian.

2. To what extent can whole sections of chromosomes be transferred from one species to another ?

To carry out this experiment it is necessary to have one or more pairs of genes exhibiting close, but not too close, linkage. It has



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recently been proved (unpublished) that the genes crinkled and green lint are rather closely linked. If, therefore, a cross is made between Upland green lint—normal, and Sea Island white lint—crinkled, and green lint—normals are always back-crossed to Sea Island crinkled we can transfer a short piece of chromosome of 5-8 units from Upland to Sea Island and observe the modifications in the Sea Island complex which result. It may be here stated that after much back-crossing it has been found possible to transfer red leaf from Asiatic to Upland. Other Asiatic genes have no doubt been transferred at the same time to some of the types, and it may be stated that some of them appear extremely promising. The Asiatic Red gene has been proved to be a new allelomorph of the red-spot multiple allelomorph series of New World cottons.

To sum up : interspecific crosses in *Gossypium* offer a wide field for experimental work on the possibilities of transferring economic characters from one species to another. It is probable that characters involving only a small number of genes could fairly easily be transferred, though the current method of utilising  $F_2$  and  $F_3$  cultures would have to be replaced by the new back-cross technique. In certain cases even the transference of a single gene might be of considerable economic importance.

PROFESSOR F. L. ENGLEDDOW, who, in the absence of Dr. Harland, introduced the paper, said that the paper was essentially a plea for further study of the interspecific genetics of cotton, more perhaps from the point of view of the geneticist than from that of the plant breeder.

In his paper, Dr. Harland refrained from dealing with the possibilities of improvement arising from crossing members of the 13-chromosome group of cottons with members of the 26-chromosome group, because his colleague, Dr. Skovsted, would discuss that in a separate paper. Another possibility was to try to cross a cotton of 13 chromosomes with others of 13 chromosomes. Dr. Harland passed that by also because, as he pointed out, his former colleague, Mr. J. B. Hutchinson, worked on this, but the work had not gone sufficiently far at the time that Mr. Hutchinson left Trinidad to judge of the possibilities. That brought Dr. Harland to the so-called New World cottons with 26 chromosomes. Of these there were seven different and now well-recognised species with 26 chromosomes, three of which were wild. Referring to the case of Egyptian  $\times$  Upland, Dr. Harland concluded that his own and other people's results made him rather pessimistic about the possibility of breeding a new cotton of any economic value from that cross.

At this point in the argument, Dr. Harland diverged a little to make an attack on what he described as the accepted methods of plant breeding, and that brought the discussion back to the previous paper, namely, the value of back-crossing as compared with straight  $F_2$ s,  $F_3$ s,  $F_4$ s and so on. Professor Engledow suggested that some misunderstanding might arise—and he felt sure Dr. Harland would readily have agreed if he had been present—unless one was very careful to state whether the argument for or against back-crosses referred to the case of one variety crossed with another in the same species, or one species crossed with another, both having the same number of chromosomes, or one species crossed with another, having different numbers of chromosomes.

As regards the case of one species crossed with another, the chromosomes of the parents being different in number, he felt sure that Dr. Harland would withdraw his remark that straightforward crossing had done no good, because one could point to the cases of wheat crosses in America, and other instances; but clearly he was not talking about that. In the case of variety crossed with variety in the same species, he also thought Dr. Harland would gladly withdraw his strictures on the accepted method, in view of the successes with English cereals which had been achieved by the straightforward  $F_1$  self-fertilised to get  $F_2$ , allowing it to self-fertilise to get  $F_3$ , and so on—in fact by the accepted method of plant breeding.

Dr. Harland's contention could best be presented in his own words: "At the present time it is idle to indulge in speculation about the value of interspecific crosses in plant breeding." He said, for example, that Upland and Egyptian cotton might be so different that they had not a single gene in common, and that he believed the chance of a success with a cross such as Egyptian and Upland was extremely remote.

Dr. Harland indicated also in his paper the two lines on which he would seek further evidence. Firstly, taking two species (and Dr. Harland was speaking of two species with the same number of chromosomes, and particularly about the  $26 \times 26$  cross; that is to say, two species within the New World group) to what extent could individual genes (in popular language, individual characteristics) of the plant be transferred from one species to another, or were the characteristics of one species and the characteristics of another species so tightly bound up within themselves that no interchange was possible?

Dr. Harland's second question was to what extent could whole sections of chromosomes be transferred from one species to another by crossing? He held out a certain hope there, for he mentioned

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that he had been able to transfer portions of a chromosome from one 26-chromosome species to another.

In both those cases Dr. Harland strongly urged the desirability of proceeding by back-crossing rather than by straight crossing and so on.

## DISCUSSION.

DR. BALLS said that when Dr. Harland spoke of the "new technique of back-crossing," he doubtless implied that it was only new in its application as a routine technique in work directed towards economic ends. Dr. Harland's point was that it was worth while as a short cut to commercial results and that sometimes it led to results that could not have been reached by other methods.

PROFESSOR ENGLEDOW said that the first paper of which he knew on the use of the back-crossing technique by plant breeders was by a man called Harlan in the early years of the War. He did not believe that that technique had developed very far, and he could not recall a case where it had proved of commercial value. He said that, however, with great reserve, because he felt very strongly the extraordinary importance of discriminating between crosses among varieties within a species and crosses between one species and another. Why inherently it should be more important to use back-crossing in one case than the other, he did not understand, and yet he felt that it must be important, because all Dr. Harland's emphasis was on the use of back-crossing between one species and another, the two species having the same number of chromosomes, whereas all the successes in plant breeding with which he was most familiar at home had undoubtedly been between one variety and another, and had been achieved without the use of back-crossing.

DR. HUDSON suggested that the difference in method to which Professor Engledow had alluded was dictated by the fact that in the straightforward breeding method only comparatively few clear cut differences between the parents were involved, whereas in interspecific crosses one was dealing with a large number of such differences. Professor Engledow replied that while he appreciated the distinction, he did not understand at present why it dictated the method.

MR. PARNELL said that back-crossing was particularly valuable if you wished to introduce one character only into a variety that in all other respects was satisfactory. He instanced some crosses made by Dr. Harland for the Station at Barberton. They had there a variety known as U.A. which by straightforward selection for hairiness they had made as resistant as possible to jassid. They also had Cambodia, which was completely resistant to jassid, but was in certain other respects not so desirable a cotton as U.A. Dr. Harland had crossed these two and back-crossed repeatedly to U.A., and it

was hoped by this means to get a cotton that retained the hairiness of Cambodia unreduced, but would possess all the other characters of U.A. Where one had two varieties neither of which was as good as it might be and it was hoped by mixing them to get something better, he did not think there was much point in back-crossing.

MR. JACKSON asked whether he was not right in thinking that in an interspecific cross self-fertilization was impossible owing to the hybrid being infertile, though it was occasionally fertile when back-crossed.

DR. SKOVSTED replied that the result of interspecific crosses between species with different chromosome numbers was not self-fertilising, but was only fertile after back-crossing on to the species with the higher chromosome number. Back-crossing was not necessary after the fourth generation: the plants were then self-fertilising and could be back-crossed on to whichever species was regarded as the more favourable. One point to be borne in mind in repeated back-crossing was that one missed any chance of getting new polyploids.

MR. TROUGHT asked if in this interspecific crossing any  $F_1$ s had been crossed between themselves, and Dr. Skovsted replied that that was a part of the problem that still remained to be tackled.

PROFESSOR HALDANE drew attention to the great variation in technical difficulty according to the species that was being handled. Thus, whereas with maize one might produce, say, 10,000 seedlings from an hour's crossing work, with sweet peas one was lucky if one could produce a dozen seedlings after working for a day. Cotton probably fell between those two extremes. If one had worked with comparatively few plants, the technical principles that limited the possibilities of breeding work in one particular plant were rather apt to assume in one's mind the status of general principles of breeding.

He suggested that there were two cases in which the technique of repeated back-crossing was particularly valuable. One was that instanced by Mr. Parnell; the other was in species crossing in which the  $F_2$  from a species is hybrid, when the progeny were definitely poor doers and of little economic value. In the case of repeated back-crossing, one set of chromosomes was retained as a going concern with the genes balanced and working together in the plant, which was consequently unlikely to fall below a certain level of vigour, whereas (speaking of plants with which he was familiar, although what he said might not be true of cotton) a good level of vigour could not be maintained if once the original chromosome complexes present in the hybrid were broken up by further selfing or crossing together of hybrid plants. He considered it important to keep separate in one's mind those two different reasons that made the method of back-crossing valuable. In certain instances, of course, both applied.



## SELECTION EFFECTS IN PURE LINES OF SEA ISLAND COTTON SELF-FERTILISED FOR SEVENTEEN GENERATIONS.

BY

S. C. HARLAND (*Cotton Research Station, Trinidad*).

### INTRODUCTION.

The Heaton variety of Sea Island cotton, grown in Montserrat, in the British West Indies, has been continuously self-fertilised since 1917. The objects of the work were to maintain the quality of the Montserrat cotton and to improve certain morphological characters bearing on yield. The work of recording was carried on by the late Mr. W. Robson, from 1917-1925, then by Mr. C. Gomez, the present Curator, and from 1930-1933 by Mr. E. C. Chin. The writer, as Assistant for Cotton Research to the Imperial Department of Agriculture for the West Indies, made recommendations about the selection work from 1917-1920, and as Cotton Adviser to the Commissioner of Agriculture has directed the work during the years 1930-1933.

From the scientific point of view the results are of extreme interest, since experiments involving so long a period of self-fertilisation have been few in number. There is also no information in the literature as to how long it takes to get a pure line in cotton, or even whether the frequency of mutation in a plant with so many chromosomes is not an insuperable obstacle to the attainment of complete homozygosity.

The whole theory of plant breeding is based on the original work of Johannsen on pure lines in beans. The conception of a pure line has been of profound importance in plant breeding, but it is now realised that in many if not all plants the attainment of a pure line is impossible, since fresh mutations will occur and again cause the line to be heterozygous.

By isolating pedigree lines of *Phaseolus vulgaris* (the common French Bean) Johannsen (1903) showed that a number of lines were produced differing in mean seed weight, and that a large number of such differing seed weights were possible. Selection of extreme plants (low and high) in both high and low lines did not cause any change in the mean seed weight of the line, and Johannsen thus enunciated the theory that selection in a pure line is ineffective.

Johannsen's work had led to great advances in the selection of improved varieties of many cultivated plants—particularly of cereals and leguminous plants. The importance of isolating what were at any rate thought to be pure lines in cotton was realised as early as 1912 by Balls, in Egypt, and his ideas were later made the basis of all breeding work with West Indian Sea Island cotton. Montserrat and St. Vincent are now planted to varieties which are purer and have been inbred longer than any other cottons in the world, though, as will be shown later in this paper, they may be considered only as relatively pure lines.

The material worked with by Johannsen was self-fertilising material, and—a very important point—he did not find one pure line, but many. It is evident that a pure line would in time produce other pure lines by mutation, and the question before us is as follows :—To what extent can selection produce measurable effects after many generations of selfing ?

It may be mentioned here that if Johannsen had really set himself to isolate hereditary variations within one of his pure lines he could probably have done it, but his method of isolating extreme variants was, in the light of our modern knowledge, not the one capable of producing results.

A pure line may produce among many hundred plants a mutation in the direction of say greater seed weight. If the effect of this mutation is only small, this plant would not be materially different from its sister plants. It would just be as likely to be at the middle of a frequency distribution as at the upper end. It would differ from the other plants, not probably in its own seed weight, but in the power to raise the mean seed weight of its offspring a little above the mean of the offspring of sister plants. Johannsen showed that selection of extreme fluctuants produced no genetic effects, but he did not show that *all* plants in his pure lines possessed the same genetic composition for seed weight. What must be done therefore is to transfer the basis of selection from the single plant to the mean of the strain. If, say, 1,000 sub-lines, each containing 50 plants, could be grown from one pure line and a frequency array compiled for mean seed weight we should get a normal curve. By selection at the upper or lower end of this frequency array of means we might make some progress if the frequency of mutation in seed weight factors were appreciable. Thus, as previously said by the writer (1919), "the measure of the worth of any plant from a selective point of view can only be obtained by expressing it in terms of the mean of its offspring."

## SELECTION EFFECTS IN PURE LINES OF COTTON.

## THE EXPERIMENTAL RESULTS.

Selection in Sea Island cotton was carried on for several years prior to 1916, but it was not until that year that the method was adopted of using only self-fertilised seed. Since 1916 twenty or more strains have been grown in each year from plants self-fertilised the previous year, and the following characters have been recorded:—1. Mean maximum lint length. 2. Mean weight of lint per seed in mg. 3. Mean seed weight. 4. Mean weight of lint per boll. 5. Mean number of boll loculi (same years).

The history of selection can be followed in the accompanying tables, and it will be of interest to follow the pedigrees from year to year and to note the changes. Some notes on the effects of selection in certain of the characters follow from this point.

*Lint Index (Weight of lint per seed).*—The weight of lint per seed, expressed in milligrams may be regarded as a most important character in cotton selection, since, other things being equal, any increase is reflected in an increase in boll weight and in increased production per acre. The writer showed (1919) that lint index was highly and positively correlated with yield per acre in the Montserrat strains. The correlation diagrams are set forth below (Diagrams 1 and 2). For the Montserrat strains of 1917 the correlation coefficient was  $0.59 \pm 0.09$ , and in 1918 it was higher still, being  $0.77 \pm 0.05$ . In view of the great fluctuations in yield brought about by environmental contingencies it is somewhat surprising to encounter such high values.

DIAGRAM 1.

CORRELATION.—LINT INDEX AND LINT PER ACRE. MONTSERRAT STRAINS 1917-1918.

|               |    | Weight of lint per acre in lbs. |     |     |     |     |     |     |     |
|---------------|----|---------------------------------|-----|-----|-----|-----|-----|-----|-----|
|               |    | 400                             | 450 | 500 | 550 | 600 | 650 | 700 | 750 |
| Lint<br>Index | 32 | 1                               | —   | —   | —   | —   | —   | —   | —   |
|               | 35 | —                               | 1   | —   | —   | —   | —   | —   | —   |
|               | 38 | —                               | 1   | —   | —   | —   | —   | —   | —   |
|               | 41 | —                               | 1   | —   | 2   | —   | —   | —   | —   |
|               | 44 | —                               | 5   | 1   | —   | —   | —   | —   | —   |
|               | 47 | —                               | 2   | 3   | 1   | —   | —   | —   | —   |
|               | 50 | —                               | —   | 2   | 1   | —   | 1   | —   | —   |
|               | 53 | —                               | —   | 1   | —   | —   | —   | —   | —   |
|               | 56 | —                               | —   | —   | —   | —   | —   | —   | 1   |

$$r = +.59 \pm .09$$

DIAGRAM 2.

CORRELATION.—LINT INDEX AND LINT PER ACRE. MONSERRAT STRAINS. 1918-1919.

|               |    | Weight of lint per acre in lbs. |     |     |     |     |     |     |     |
|---------------|----|---------------------------------|-----|-----|-----|-----|-----|-----|-----|
|               |    | 450                             | 500 | 550 | 600 | 650 | 700 | 750 | 800 |
|               |    |                                 |     |     |     |     |     |     |     |
| Lint<br>Index | 35 | 1                               | —   | 1   | —   | —   | —   | —   | —   |
|               | 38 | 1                               | —   | —   | —   | —   | —   | —   | —   |
|               | 41 | —                               | 1   | —   | —   | —   | —   | —   | —   |
|               | 44 | —                               | —   | 1   | 1   | 1   | —   | —   | —   |
|               | 47 | —                               | —   | 2   | 1   | —   | —   | —   | —   |
|               | 50 | —                               | —   | 1   | —   | 4   | —   | —   | 1   |
|               | 53 | —                               | —   | —   | 1   | 2   | —   | —   | —   |
|               | 56 | —                               | —   | —   | —   | 1   | —   | —   | —   |
|               | 59 | —                               | —   | —   | —   | —   | —   | 1   | —   |

$$r = +.77 \pm .05$$

The value of lint index as a factor in selection has been under-estimated in countries where the yields are low, and where other factors are regarded as of major importance. An increase of ten per cent. in yield derived from an improvement in lint index is of course more spectacular where the yield is already high, but even in environments characteristically low-yielding an increased yield of ten per cent. may make all the difference between profit and loss. An increase in lint index may be due to an increase in the number of hairs per seed, to an increase in hair weight, or to both these factors combined, and mere selection of high lint indices regardless of lint length or fineness would be unwise, since it has been shown that high lint length is incompatible with high lint index.

Prior to 1916 no particular selection for this character was practised in the Heaton type, and lint index ranged from 32 to 59 mg. per seed in that year. It was realised however that the material was heterozygous for lint index factors and as a result of selection for two years the mean lint index was raised from 51 to 60 mg. The results of selection for two years are placed below :—

|          |          | Weight of lint per seed. |    |    |    |    |    |    |    |    |    |    |    |    |       |
|----------|----------|--------------------------|----|----|----|----|----|----|----|----|----|----|----|----|-------|
|          |          | Year.                    | 35 | 38 | 41 | 44 | 47 | 50 | 53 | 56 | 59 | 62 | 65 | 68 | Mean. |
| Original | H 9-23 { | 1916                     | 1  | —  | —  | 4  | 8  | 5  | 11 | 7  | —  | 2  | —  | —  | 51    |
|          |          | 1918                     | —  | —  | —  | —  | —  | 2  | 1  | 8  | 13 | 18 | 7  | 1  | 60    |



## SELECTION EFFECTS IN PURE LINES OF COTTON.

In 1918 the lowest value for lint index was 50 and the highest 68. Subsequently self-fertilisation has been practised in this strain (H9—23—2—13) for 15 generations making altogether 17 generations of selfing with the exception of one strain, unselfed in 1922. It is impracticable to present the frequency arrays in full.

The frequency arrays of the *means* of the strains are presented in Table 1. In 1917 the highest strain of the year had a mean of 56. By selection this was raised to 60 in 1918, to 63 in 1919, and to 64 in 1920. Subsequently from 1921-1930, out of 149 strains grown, 3 reached a mean of 63. In 1931 80 strains were grown from the 6 strains which possessed the highest means in 1930, and five of them were 64 and above. In 1932 this process was repeated until in 1933 we have 47 strains of 65 and above, reaching in the highest strain the enormous value of a mean of 72 mg. of lint per seed. Thus, after thirteen generations of selfing, intensive selection from 1930-1933 has resulted in a great increase in lint index. In 1919, a single strain showed a mean of 63, and 51 plants from this strain were grown in progeny rows in 1920. The means ranged from 51 to 64 in the 51 strains. In 1930 no strain was higher than 59, yet 80 daughter strains from the 80 highest values in the 6 strains of values 57 to 59 gave higher values than the 51 strains of 1920. It may be assumed that in the period from 1920 to 1930, mutation has taken place in genes affecting lint index. These have been perpetuated in the selections and became available for isolation by the intensive selection practised in 1931 to 1933.

It is of interest to follow in detail the line of descent of the highest strain of 1932 back to 1916. This is done in Table 2, in which the data are complete with the exception of 1922 when the selfed seed was lost and unselfed had to be grown. It will be seen that the mean from 1917-1930 has varied between the comparatively narrow range of 56 to 63, ignoring the exceptionally low value (47) of 1920, probably due to the examination having been made on selfed seeds from the last ripening bolls in the plants in which lint weight was much reduced. Sometimes the selected plant as in 1919 and 1924 was at the top of the previous years' frequency arrays; sometimes as in 1925, 1926 and 1927 plants from near the lower end of the frequency distribution were selected.

Nevertheless by selecting only from rows which had high means there appeared in 1931 and 1932 plants of lint index 71 and 74, higher than had previously been recorded in Sea Island cotton.

For purposes of record a number of lines of descent for lint index and lint per boll are presented in Tables 3 and 4. The great advance in weight of lint per boll due to the increase in lint index will be observed.

Recording of individual plants is not now practised; the unit of selection has been transferred to the *mean of the strains*.

Some general considerations may now be affirmed. It is now generally believed that many if not most quantitative characters in living organisms are determined by a large number of genes. Such characters as lint index, seed weight, and lint length in cotton are probably affected by up to 200 to 300 genes, and in an ordinary population exposed to some natural crossing a large number of such factors can be present in a heterozygous condition. Following the argument of "Student" (1933), "In ordinary times these (genes) would roughly neutralise one another, each individual carrying a mixture of genes which would produce variations in opposite directions, so that only a limited genetic variation would result; but with a change of environment this reservoir of genes would serve a very useful purpose as raw material for selection; some characters formerly neutral would then affect survival. . . . Thus the accumulation of small variations in the same direction could proceed far beyond the original range."

The above argument is borne out by the results of the experiments on selection for high lint index dealt with in this paper. The great increase, manifest since 1930, consequent on intensive selection signifies either that the original strain of 1916 was enormously heterozygous or that numbers of plus mutations have occurred in the direction of higher values. The upper limit of selection does not yet appear to have been reached and it is probable that still further progress can be made.

From the point of view of the plant breeder the important points would appear to be as follows:—

A large number of generations of self-fertilisation are necessary before strains of cotton can be said to be pure. The term "pure" should therefore be used only with extreme caution.

Since positive effects from selection have been demonstrated to occur after many generations of selection in the Montserrat material, the methodology of cotton selection should in the future take note of the fact that continuous selection in strains considered pure is likely to be effective.

## SELECTION EFFECTS IN PURE LINES OF COTTON.

It may be affirmed that *continuous selection* for disease resistance, for improvement of agricultural properties, and for improvement of lint quality may be of enormous importance to cotton-growing countries. Improvement however is only practicable where large number of strains are grown and *where the mean of the strain is regarded as the unit for selection* instead of the single plant value.

## ACKNOWLEDGEMENTS.

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TABLE 1.  
FREQUENCY ARRAYS OF MEAN LINT INDEX OF THE MONTERRAT STRAINS, 1917-1933

| Year. | No. of<br>Strains | 45 | 46 | 47 | 48 | 49 | 50 | 51 | 52 | 53 | 54 | 55 | 56 | 57 | 58 | 59 | 60 | 61 | 62 | 63 | 64 | 65 | 66 | 67 | 68 | 69 | 70 | 71 | 72 | Mean. |
|-------|-------------------|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|----|-------|
| 1917  | 9                 | 3  | —  | 1  | 1  | 2  | —  | —  | 1  | —  | —  | —  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 43    |
| 1918  | 6                 | —  | —  | 1  | —  | 1  | —  | —  | 1  | —  | —  | 1  | 1  | —  | —  | —  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 53    |
| 1919  | 4                 | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | —  | —  | 1  | 1  | —  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 60    |
| 1920  | 51                | —  | —  | —  | —  | —  | 1  | —  | 2  | 4  | 4  | 3  | 7  | 6  | 5  | 7  | 5  | 4  | 1  | 1  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | 57    |
| 1921  | 19                | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 2  | 2  | 2  | 2  | 7  | 4  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 59    |
| 1922  | 20                | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 1  | 5  | 5  | 3  | 3  | 2  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 56    |
| 1923  | 25                | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 2  | 2  | 6  | 2  | 7  | 2  | 1  | 3  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 53    |
| 1924  | 20                | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 1  | 3  | 5  | 4  | 3  | —  | 2  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 58    |
| 1925  | 10                | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 2  | 2  | 1  | 1  | 2  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 60    |
| 1926  | 12                | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 4  | 3  | 1  | 1  | 1  | 1  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 56    |
| 1927  | 20                | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 3  | 3  | 3  | 5  | 2  | 1  | 2  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 50    |
| 1928  | 17                | —  | —  | —  | —  | —  | 1  | 1  | 1  | —  | 2  | —  | 1  | 3  | —  | 4  | 3  | —  | 2  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 58    |
| 1929  | 26                | —  | —  | —  | —  | —  | —  | —  | 1  | 1  | 4  | 3  | 8  | 1  | 5  | —  | 1  | 2  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 56    |
| 1930  | 6                 | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 3  | 2  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 58    |
| 1931  | 80                | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 1  | 4  | 3  | 17 | 9  | 12 | 10 | 12 | 6  | 3  | 2  | —  | —  | —  | —  | —  | —  | —  | 60    |
| 1932  | 20                | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | —  | —  | 1  | 4  | 3  | 4  | 3  | 2  | —  | 2  | —  | —  | —  | 65    |
| 1933  | 60                | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | —  | 1  | 1  | —  | 2  | 4  | 5  | 10 | 15 | 11 | 6  | 3  | 1  | —  | 1  | 66    |



## SELECTION EFFECTS IN PURE LINES OF COTTON.

TABLE 2  
SHOWING LINE OF DESCENT OF STRAIN WITH HIGHEST LINT INDEX IN 1932.

| Strain.  | Parent | Year | 35 | 38 | 41 | 44 | 47 | 50 | 53 | 56 | 59 | 62 | 65 | 68 | 71 | 74 | Mean |
|----------|--------|------|----|----|----|----|----|----|----|----|----|----|----|----|----|----|------|
| 11 9 -23 | 50     | 1916 | 1  | —  | —  | —  | —  | —  | —  | ×  | —  | —  | —  | —  | —  | —  | 51   |
|          | 56     | 1917 | —  | —  | —  | 4  | 8  | 5  | 11 | 7  | ×  | 2  | —  | —  | —  | —  | 56   |
|          | 60     | 1918 | —  | —  | —  | —  | 1  | 2  | 6  | 5  | ×  | 1  | ×  | 1  | —  | —  | 60   |
|          | 64     | 1919 | —  | —  | —  | —  | —  | 2  | 1  | 8  | 13 | 18 | ×  | 1  | —  | —  | 63   |
|          | 65     | 1920 | —  | —  | —  | —  | —  | —  | —  | —  | 8  | 11 | 11 | —  | —  | —  | *47  |
|          | 56     | 1921 | —  | 2  | 4  | 6  | 5  | 6  | 2  | 2  | 2  | —  | —  | —  | —  | —  | 61   |
|          | —      | 1922 | —  | —  | —  | —  | —  | —  | 2  | 1  | 8  | 11 | 4  | 1  | —  | —  | 56   |
|          | —      | 1923 | —  | —  | —  | —  | 1  | 2  | 17 | 65 | 29 | 3  | —  | —  | —  | —  | 59   |
|          | 62     | 1924 | —  | —  | —  | —  | 1  | —  | —  | 8  | 13 | ×  | 1  | —  | —  | —  | 59   |
|          | 64     | 1925 | —  | —  | —  | —  | —  | —  | 1  | 5  | 7  | 8  | 3  | —  | —  | —  | 62   |
|          | 55     | 1926 | —  | —  | —  | —  | —  | —  | —  | ×  | 8  | 9  | 9  | 2  | —  | —  | 57   |
|          | 55     | 1927 | —  | —  | —  | —  | —  | —  | 1  | 18 | 8  | 8  | —  | —  | —  | —  | 57   |
|          | 54     | 1928 | —  | —  | —  | —  | —  | 2  | 8  | 8  | 6  | 6  | —  | —  | —  | —  | 62   |
|          | 62     | 1929 | —  | —  | —  | —  | —  | —  | —  | 1  | 17 | ×  | 6  | 1  | —  | —  | 58   |
|          | —      | 1930 | —  | —  | —  | —  | 1  | —  | 1  | 11 | 11 | 1  | 2  | 2  | —  | —  | 59   |
|          | 58     | 1931 | —  | —  | —  | —  | —  | —  | 3  | 7  | ×  | 9  | 1  | —  | —  | ×  | 63   |
|          | 74     | 1932 | —  | —  | —  | —  | —  | 2  | —  | 5  | 6  | 4  | 1  | 4  | 1  | 1  | 66   |
|          |        |      | —  | —  | —  | —  | —  | —  | —  | 1  | 1  | 2  | 14 | 9  | 3  | —  |      |

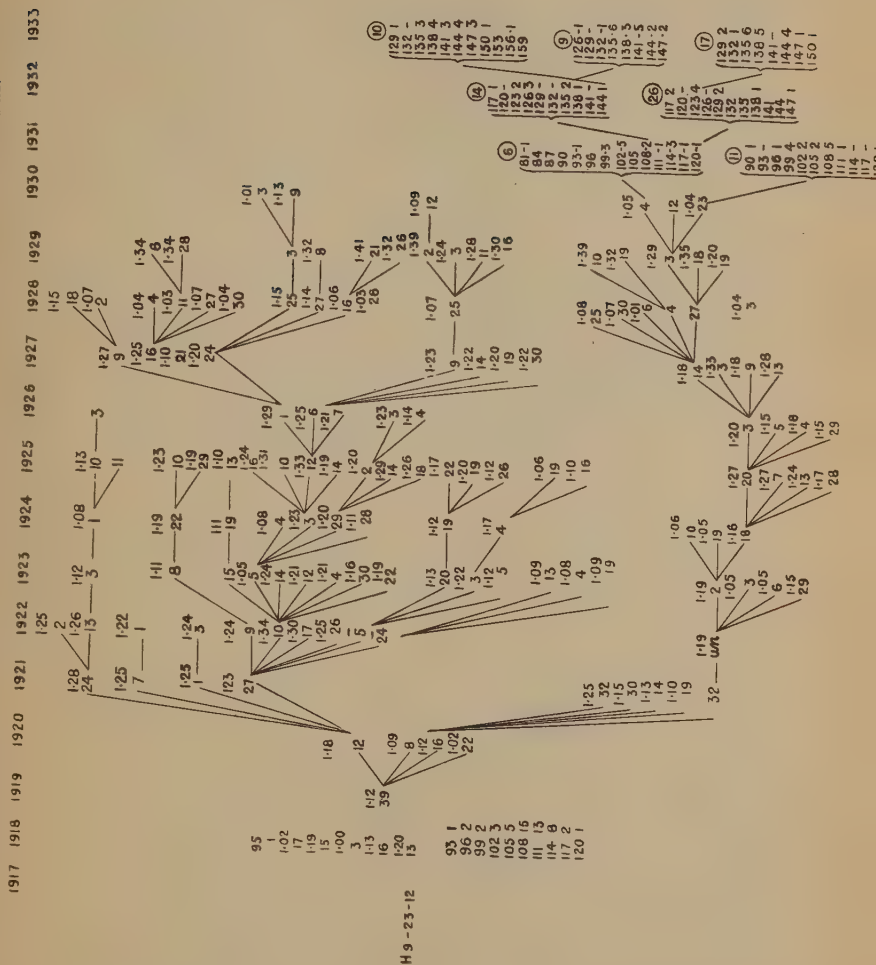
\* Bulk sample gave value of 57.

× Parent value of strain.

JOINT INDEX. CHART OF PEDIGREE CULTURES OF SEA ISLAND COTTON IN MONTSERRAT.

NOTE.—Up to 1930 the lower number is the strain number and the upper number is the frequency arrays of 1000.

umber the lint index value. Subsequent to 1930 only  
s are presented.



In introducing Dr. Harland's paper in the author's absence, MR. EVELYN said that the principal points that it was designed to bring out were as follows: (1) "A large number of generations of self-fertilisation are necessary before strains of cotton can be said to be pure. The term 'pure' should therefore be used only with extreme caution." (2) Since positive effects from selection have been demonstrated to occur after many generations of self-fertilisation in the Montserrat material, the methodology of cotton selection should in the future take note of the fact that continuous selection in strains considered pure is likely to be effective; and (3) That from a selective point of view, the value of a strain can only be measured by the *mean* of its progeny. Of these three it was the second with which the paper chiefly dealt. That is that positive effects were obtained from selecting for high lint index within a strain of Montserrat Sea Island that had been self-fertilised for seventeen generations.

Results similar to those described in the paper had been obtained in St. Vincent by selection. The strain V.135 (Superfine Sea Island) had been self-fertilised there since 1919. Its original lint length had been 57 mm. Selection, restarted in 1930, had in two seasons raised the mean length to 59 mm. and certain individual strains to 60 mm. and over.

Again, in Carriacou, where perennial cottons had been grown since 1831 and where selection, although discontinuous, had been done, some strikingly long linted strains had recently been obtained by selection.

Mr. Evelyn suggested that some cotton workers were too easily satisfied with their results from selection. Thus, if for instance the lint length desired had been obtained, the strain was frequently multiplied up and distributed to the growers. Scarcely any further work, other than that of maintaining this strain pure for length of staple, was then carried out on it, the assumption being made that self-fertilisation of a strain for, say, ten years would result in the purification of all of its characters. He considered that pure lines should be carried much further with the object of discovering how far other characters within a line which was pure for a specific character could be improved without detriment to that specific character. As an illustration of a piece of work of this kind, Mr. Evelyn said that after Dr. Harland had isolated V.135 (pure for a 57 mm. length), he started selecting within the line for resistance to Angular Leaf Spot. After a few years he obtained a strain highly resistant to Angular Leaf Spot, in which the length and quality of the lint were in no way different from his original pure line. Also, since 1932, selection had been

## SELECTION EFFECTS IN PURE LINES OF COTTON.

made within V.135 for high number of bolls per tree, and it had been found possible to raise the mean number of bolls per tree from 50 to 70 without impairing the value of either the length and quality of the lint or the high resistance to Angular Leaf Spot. Such results, he submitted, justified continued selection within pure lines, not only for one specific character, which it had been shown could be improved even after 17 generations of self-fertilisation, but for other characters of equal agricultural importance. Although there was a negative correlation between the characters lint length and lint index in V.135, yet it was possible to improve the latter by selection up to a point without detrimentally affecting the former.

## DISCUSSION.

THE CHAIRMAN asked whether Mr. Evelyn was quite certain that his material was entirely self-fertilised, to which Mr. Evelyn replied in the affirmative, saying that the flowers were all tied with twine.

Later in the discussion Mr. Hosking said that in Uganda that method was not satisfactory, because there was a beetle which drilled a hole through the petals and got into the flower. Under Uganda conditions self-fertilisation could not be guaranteed unless every plant was examined, which was impossible.

MAJOR CAMERON suggested that Mr. Evelyn's statement that plant breeders tended to select for one character to the neglect of others, was too sweeping; in his experience plant breeders all looked for a balanced plant. There was considerable difficulty in selecting for one character and keeping others going at the same time. As an example, he mentioned that by straight selection they had been able in Southern Rhodesia to produce from U.A cotton a plant which was as hairy as the original Cambodia, but at the same time the early maturing character of U.A was lost.

Referring to the extreme difficulty of obtaining a really pure strain, Major Cameron suggested tentatively that the word might be abandoned, and some other such word as "pedigree" substituted.

SIR GEOFFREY EVANS supported Major Cameron in his statement as to the difficulty of obtaining purity in a strain. When he arrived in Queensland some years ago with W. G. Wells, who was at the present time in charge of cotton research work in Queensland, they found that with a view to establishing a cotton-growing industry, the authorities had imported various types of cotton from all over the world, and had let them get mixed. It was therefore necessary to make a fresh start. It was known that in certain conditions and in certain localities the American type would grow very well. The problem was to obtain a supply of the right type of seed. In Queensland—and indeed in Australia generally—the conditions probably differed from those in the rest of the world. If either a plant



or an animal were imported, it seemed to grow quite differently from the way in which it grew in its normal habitat, and that was certainly true of cotton. As an instance of what was called "new place effect," it was rather astonishing to see what happened to some of the "pure lines" of cotton that were obtained from America. One of the types introduced was a cotton called Webber 49, which had a staple length of slightly over an inch. It was a pure line, guaranteed by Webber himself. It was selfed in Queensland for three generations, and in the third generation it was practically what the Americans call slick-seeded, i.e. naked and without fuzz.

Again, some Cambodia was imported from Madras, guaranteed selfed for several generations. In the first year they got some tall plants, some hairy, some small and some smooth, yet he had every confidence that the Madras Department had sent the right seed.

The third cotton was a cotton called Durango, which had since become the staple cotton of Queensland. It was a type which suited local conditions very well. Pure seed was obtained from one of the best experimental stations in Arizona, but in the first generation it split up into all sorts of types, so it was necessary, by straight line breeding, to select the types required. It settled down in three years, and the present type was probably not classed on the Liverpool market as Durango at all, though it originally came from a pure line of that cotton.

Sir Geoffrey said he mentioned those facts to show that the ordinary so-called pure lines of cotton—at any rate the commercial pure lines, and even pure lines from research stations in America—were far from being homozygous.

MR. WATKINS pointed out that from Table 1 in the paper, it seemed that, starting in 1917, with material which evidently was not pure, it had been possible in the first two or three generations to get an immediate effect as a result of selection, as one would have expected. Then, from 1919 and for the next 13 generations or thereabouts, selection produced no effect whatever. Then, somewhere about 1930 or 1931, the effect first showing itself in 1932, there was a sudden change, and selection produced an effect. He asked if Mr. Evelyn could explain the sudden change, which showed itself in 1932 after 13 years of apparent constancy.

MR. EVELYN, in reply, said that the hiatus was due to the fact that during the years referred to selection for lint index was not actually taking place. All plants with a lint length of 57 mm. and upwards were classed together as more or less maintaining the stability of the strain. The increase in 1930-31 coincided with the appointment of a Cotton Officer in Montserrat.

In reply to Major Cameron, Mr. Evelyn said that the point he wished to make was that in selecting for a certain character he thought it was essential, if optimum results were to be obtained for that character,

## SELECTION EFFECTS IN PURE LINES OF COTTON.

*not to be side-tracked into selecting other good characters at the same time. Having stabilised the character required, the strain could then be improved by selecting for other required characters within that stable line. If an attempt were made to select for several characters at the same time, one was apt to get no results.*

MR. JACKSON and MR. PARNELL suggested that one way of achieving Mr. Evelyn's object was that, in selecting for length, all plants not conforming to a standard in other factors should be discarded. Mr. Parnell further suggested that by concentrating on one character only, the plant breeder would be very apt to lose many of the other valuable characters contained in the strain. This had been brought out in the example given by Major Cameron. He advocated strongly that all the characters should be watched continually and constantly.

MR. EVELYN asked to be allowed to qualify his statement. Although conditions in St. Vincent were exceptional inasmuch as, growing the longest cotton in the world, their main object was to maintain that length, nevertheless selection was based on the mean value of the staple length of the progeny and not on the value of individual plants. The other characters of agricultural importance were contained in a heterozygous condition in the plants of the row selected, and thus were not lost, but were always in the material, awaiting isolation whenever required. Most of the alleged pure material received at the Station was generally pure for one character, but definitely heterozygous for quite a number of others.

PROFESSOR BRITON-JONES asked whether the falling away of a variety might not, in some cases, be due to malnutrition.

MR. EVELYN, in reply, said that in his experience conditions of poor soil and bad cultivation would shorten and coarsen lint and deteriorate yield, but if the strain were relatively pure for those characters, a return to good conditions would demonstrate that they had not been lost. The actual genes had not altered under poor conditions: it was only that they had not been able to express themselves fully.

PROFESSOR ROBERTS, speaking from the grower's point of view, said that if a strain after seventeen generations of self-fertilisation, could still show the possibility of so remarkable an increase in staple length, he felt some doubt as to whether the material described in the paper could be regarded as reliable, except for certain characters.

Referring to Mr. Watkins' paper, Professor Roberts said that from a short-range point of view, he was of opinion that selection offered by far the most rapid and certain method of getting results. He did not, however, wish to say anything that might be regarded as deprecating the application of hybridisation from a long-range standpoint.

DR. BALLS said that he would like to repeat that the practical point of view was that a so-called pure line was to be regarded as pure

if it could not be shown to be impure, and the more discriminating the tests that were applied, the more it could be split down to greater purity. Considerably more than half the Egyptian crop was grown by seed renewal out of such reputed pure lines, a matter of well over half a million bales per annum. It should always be remembered that the genetic make-up of commercial cotton, the material or the crosses from which selection was made, was intensely complicated. For example, there was evidence that the gene make-up of the Upper Egyptian cotton was quite different from that of the Delta cottons, yet both were varieties of the same cotton.

Ordinary cotton was subjected to a small amount of open fertilisation, but in the course of many generations that resulted practically in an open-fertilised crop. In that there was an enormous mixture of genes, the majority of which were invisible, being physiological in their effects. One of the things that they modified was the lint length. It was possible to discriminate, mentally at least, between a morphological gene and a physiological one, and so envisage breeding a pure line morphologically, which is subject to the action of these modifying factors. As methods of discrimination became more delicate, splitting could be carried further and a morphological 57 mm. staple produced which had a physiological modifier also bred pure with it, thus carrying it up to 62.

He would be sorry to see the term "pure line" dropped, as Major Cameron had suggested, not because it was accurate, but because it represented an ideal which, moreover, was not unattainable. Also, something short of the ideal was most effective in working practice for providing stability in seed supply.

PROFESSOR HALDANE said that it seemed to him from the discussion that cotton breeders used the term "pure line" for strains which did not conform to the stricter definition that would be required by a geneticist, and suggested that a rather more rigorous definition of the term should be required.

## CYTO-GENETICS IN RELATION TO PLANT BREEDING IN COTTON.

BY

A. SKOVSTED (*Cotton Research Station, Trinidad*).

The object of this note is to present a survey of the cyto-genetical facts so far discovered in cotton and in other species of the genus *Gossypium*, and to illustrate their significance to the cotton-breeder.

Cytological investigations have shown that the differences between the two main groups of cultivated cottons, indigenous to the New and Old Worlds respectively, are associated with a difference in chromosome numbers: Old World cottons have 13 pairs of chromosomes, while New World cottons have twice as many, *i.e.*,

26 pairs. A few of the wild species (*G. tomentosum*, *G. taitense*, and *G. Darwinii*) are closely related to New World cottons, and have 26 pairs of chromosomes. The majority of the wild species, however, both from the Old and the New World all have 13 pairs of chromosomes.

It is known from other organisms that a complete pairing of chromosomes during the phases preceding the actual separation in the reduction division is essential for the production of full fertility; irregular pairing, on the other hand, leads to sterility.

All Old World cottons are known to be inter-fertile, and it has been shown cytologically that the chromosome pairing is normal in a hybrid between the species *G. arboreum* and *G. herbaceum*. New World cottons have also been shown to be practically completely inter-fertile, though cytological studies of them are yet incomplete.

Hybrids between the two main groups of cultivated cottons have been difficult to produce. When obtained, they have proved very largely, though not entirely, sterile. Cytologically, this sterility was to be expected since the hybrids have the uneven number of 39 chromosomes. Cytological studies of hybrids between Old and New World cottons show that the chromosomes of Old World cottons are able to pair with half of the chromosomes from New World cottons, and that the remaining half, which are quite dissimilar, are left as univalents.

Two other hybrids with New World cottons which have the same chromosome number (39) have been produced and made the subject of cytological studies. They are hybrids between cultivated New World cottons and two wild species from America: *Gossypium aridum* (generally regarded as a separate genus *Erioxylum*) and *G. Harknesii* (both with the chromosome number 13). In these two hybrids the conjugation of the chromosomes is similar. This type of conjugation is, however, entirely different from that of the hybrids between Old and New World cottons. The conjugation of the chromosomes in the above-mentioned two hybrids, taken in conjunction with their size, leads one to infer that the chromosomes of the wild species conjugate with that set of 13 chromosomes from New World cotton which remained as univalents in the hybrid between Old and New World cottons. The hybrid with *G. aridum* is found to be slightly fertile, and preliminary experiments with the *G. Harknesii* hybrid indicate that this too is probably slightly fertile. A third wild species from America, *G. Thurberi* (with 13 pairs of chromosomes) (*Thurberia thespedioides*, A. Gray) has also been found to be slightly fertile when hybridised with New World cottons. However, it has not as yet been cytologically examined.



It is possible both from a cytological and a genetical point of view to substitute whole chromosomes or parts of chromosomes in one of the species of Asiatic cotton with the corresponding chromosomes or parts of chromosomes from the other species of Asiatic cotton. Cytologically, it now appears possible to make similar substitution in half the chromosomes of a New World cotton with the similar set of chromosomes from an Asiatic cotton. Moreover, the same process would appear possible between the remaining half of the chromosomes from New World cotton and the chromosomes from the above-mentioned wild species from America.

These three wild species from America are either lintless, or possess very short lint, so that the characters which it may be possible to extract from them will in all probability not be valuable lint characters. On the other hand, these three wild species are xerophytes, and it may be of economic importance to introduce their characters into New World cotton when breeding types suitable for dry climates. Again, the greater immunity to diseases inherent in the wild species may be of special interest to the plant-breeder, *i.e.*, it may be mentioned that under Trinidad conditions *G. Thurberi* appears to possess complete immunity from attacks of pink bollworm. However that may be, studies of the first backcross of Sea Island  $\times$  *G. aridum* to Sea Island shows that it is at least possible to combine in the same plant some characters from the wild lintless species with long lint characters from the cultivated species. The first nine plants produced were found to possess characters definitely from *G. aridum*, while the lint-length varied between 31 and 49 mm. (The Sea Island cotton which was used had a lint-length of 50 mm.).

Another cyto-genetical problem of a somewhat different nature is the formation of new polyploids. Such types may possibly be produced both from species hybrids and from the pure species. This increase in chromosome number may occur in one of two different ways: (1) in the vegetative cells; or, (2) in the pollen or egg cells. A vegetative duplication of the type so well known from the *Solanaceae* has only been found to occur once in cotton, *i.e.*, in an Asiatic cotton where the number of chromosomes was duplicated from 26 to 52. Unfortunately the duplication had taken place in a root and not in the stem of the plant.

Chromosome duplication has been observed to have taken place once in a pollen cell and once in an egg cell of Asiatic cotton. The chances of two such gametes meeting are so small that it appears extremely unlikely that a tetraploid Asiatic cotton may be produced in that way. An increase in the number of chromosomes has several times been observed in gametes derived from the interspecific hybrids



with 39 somatic chromosomes. Gametes which could function have been found to have chromosome numbers ranging between 26 and 39; and in one instance a gamete which functioned must have had c.64 chromosomes, since the plant which resulted from the fusion of this gamete with an egg from Sea Island cotton was found to contain c.90 chromosomes. This plant was extremely weak and never flowered, though its nine sister plants which had c.52 chromosomes were fairly vigorous and all partly self-fertile. This illustrates the fact that it is not sufficient merely to increase the chromosome number. Each chromosome must have a partner, and the sets of chromosomes must be present in such proportions that the plants are physiologically balanced. It appears, then, that the most promising polyploids are those which would arise as the result of a duplication in somatic tissue, since such types are both physiologically and cytologically balanced. Unfortunately, with our present knowledge, we possess no methods wherewith to speed up the chromosome duplication in the somatic tissue of a cotton plant.

The object of the present note has been to supply a cytological outline of the material which our present knowledge denotes as interesting to the plant-breeder of cotton. It seems (1) that the different types of wild Asiatic cottons (with 26 chromosomes) may be used in breeding new types of Asiatic cottons; (2) that the different types of New World cottons (with 52 chromosomes) may also be used in breeding fresh types of New World cottons (as has been emphasised by Dr. Harland in the *Empire Cotton Growing Review*); and (3) that the fertile hybrids between the two main groups, together with the fertile hybrids between New World cottons and the wild species from America, offer a promising working-field to plant-breeders.

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In introducing his paper, DR. SKOVSTED said that its object was to give a very short account of some of the cytogenetic facts that had been observed in cotton and other species of *Gossypium*. It was also hoped that it might be a means of directing the interest of practical plant breeders to some of the fertile hybrids between cultivated New World cottons and wild species of *Gossypium*. Some of the hybrids appeared promising material for purely economic purposes.

#### DISCUSSION.

PROFESSOR HALDANE asked whether workers in this country could say something as to the value or otherwise in wheat breeding of transferring characters from a group with one chromosome number to another with a different chromosome number. For example, whether they considered that the attempt to transfer characters from a 28-chromosome wheat to a 42-chromosome wheat had been valuable not merely scientifically, but economically.

MR. WATKINS said that American workers had been successful in transferring the resistance to *P. graminis*, which was found among certain tetraploid wheats to some of the hexaploids which were commonly grown for bread-making. There seemed to be no theoretical reason why it should not be possible in some other cases in wheat to transfer characters from one group to another, although there might be practical difficulties.

Similar transfers might be rather more difficult with cotton than with wheat, because the fact that sterility was much higher indicated the possibility of underlying difficulties.

Mr. Watkins asked whether any of the wild American species of cotton, which were not likely themselves to be of economic value, had any economically useful attributes. It might perhaps be possible to synthesise something new, starting with these wild forms and using, perhaps, some of the Old World cottons. If the synthesis of new forms were possible, it would give an opportunity of getting a much increased variation by crossing the new forms with the existing cultivated forms.

DR. SKOVSTED replied that some of the wild species came from extremely dry areas, and it was therefore of interest and importance to try to breed from them varieties that were economically useful and would also stand up to an extremely dry climate. He thought this might be possible.

Another important point was that some of the wild species of New World cotton seemed to be very resistant to most of the diseases that occurred in Trinidad, at all events, and he thought it was well worth while to investigate the possibility of making use of those species as material for breeding resistant commercial types.

MR. JACKSON said that he thought there were immense possibilities in such crosses as those referred to by Dr. Skovsted. He would also like to emphasise the extreme importance of the root system of the cotton plant in relation to its general physiology and in particular to disease resistance. He thought it possible that some of the wild types which had resistant characteristics might owe them to their root system, and the introduction of such a characteristic might be brought about by transferring a section of chromosomes or genes regulating the root system. He suggested that the study of root exposures might throw light on this matter, though he was aware that such work was very tedious and required a large staff.

PROFESSOR CHEESMAN said that the potentialities of the type of work under consideration seemed to him to be so great as almost to preclude discussion at the present stage. He considered that it was precisely the type of work—and possibly the only type of work—that would lead to much more fundamental knowledge of the make up of cultivated cottons.

## CYTO-GENETICS IN RELATION TO PLANT BREEDING.

*Referring to the possibility of raising new polyploids, his own work had been mainly with bananas, and while aware of the danger of arguing from one genus to another, he submitted that fairly wide crosses between species often led to increases in the chromosome number, and were perhaps the most certain means of obtaining new polyploids. It remained to be seen what could be done in that way with cotton, and Dr. Skovsted's work was, in his opinion, the way in which it would be found out.*

PROFESSOR ENGLEADOW said he felt hopeful about interspecies crosses and interspecies transferences under certain conditions. Mr. Watkins himself had crossed one of the best types of hexaploid wheat with one of the very best types of tetraploid and got an immense variety of things, but he (Professor Engledow) had failed to get anything of economic promise from that material. That experience had made him cautious, because Mr. Watkins had handled a considerable amount of material.

*In the United States of America they were concerned with resistance to stem rust, and they had found that their 14-chromosome tetraploid wheats were more resistant than the others, but the 14-chromosome wheats, though excellent for macaroni, were very bad for bread: the loaf did not rise well and the flour was yellowish. However, rust resistance being so important, they had crossed the macaroni and bread types, and produced a wheat which they called Marquilla—a combination of the two names. It had a much greater resistance to rust than the Marquis wheat, but when it came to be put on a large scale basis, it could not be taken up by the trade, because the fatal yellow colour of the macaroni parent had not been eliminated.*

Professor Engledow said he quoted those two cases merely to show that, while we must all have faith in the ultimate possibilities, the enormous difficulty of handling economic characters should not be lost sight of. Unfortunately, the economically useful characters were often extraordinarily complex, being made up of the effects of a large number of genes. He thought that fact might prove a great barrier to progress in the application of genetics to plant breeding all round.

## PRELIMINARY TESTING OF NEW VARIETIES OR TYPES OF COTTON.

BY

T. TROUGHT (*Empire Cotton Growing Corporation*)

AND

H. E. KING (*Sudan Government*).

## INTRODUCTION.

This note describes a method which has been developed by Mr. M. A. Bailey and the staff of the Plant Breeding Section at Shambat during the last few years, in order that quantitative

comparisons can be made, when only very small quantities of seed are available, between a selection and standard type, and between different new selections.

*Need for such Test.*—In plant breeding, with a plant such as cotton in particular, considerations of area, staff and labour demand that propagation and testing proceed simultaneously. The difficulty in the early stages is to know what to select for propagation. The yield of a single plant bears little relation to the yield per acre of that plant's progeny.\* Field and laboratory notes assist the breeder in making guesses, which are sometimes successful.

The value of a quantitative test which can be applied in the second year after the selection of a single plant, and which gives strong indications of the productive capacity per acre of the plant, is obvious.

*Resumé of Method.*—The seed from a promising "family" (single plant progeny) is bulked, the natural seed being kept separate from any self-bred seed which may have been collected. The self-bred seed forms the nucleus for the propagation of the type for seed. The natural seed is used partly for propagation or large-scale replicated tests, and part (the smaller part) is used for a "Line Test." If the family is not sufficiently purified to be bulked for the first stage of propagation, the seed from six plants only of the desired type will give enough seed for a preliminary line test, while further single plant selections are made in the family.

Generally 1 lb. of seed may be expected from a bulked family of fifty plants (American types give more at Shambat). This suffices for a one-eighth acre propagation plot (wide spaced) and a 100-metre line or ridge, sown in holes 45 cms. apart at 4 to 5 seeds per hole.

The layout consists of a series of "triplets" of lines or ridges, the central ridge being sown with the variety being tested, and the two adjacent ridges with the control types. Between each triplet is a "division" line, sown also with the control type. Great care

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\* Cf. "Parent progeny correlations in Cotton" by J. F. O'Kelly and W. W. Hull. *Journ. Am. Soc. Agron.* Vol. 25 No. 2 (Feb. 1933).

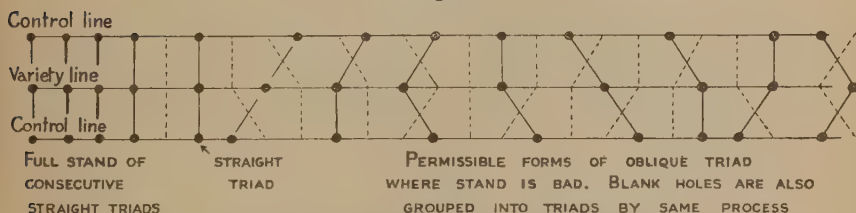
## PRELIMINARY TESTING OF NEW VARIETIES.

must be taken that the ridges or lines are exactly parallel, and that they are the exact distance apart. Equal care must be taken with the distance between holes along the ridge.

After sowing and germination, the first re-sowing is done as early as possible. It is at this stage that the first "equalization" is done. Equalization is the essence of the method, and, as its name implies, ensures that the control ridges and varietal ridge are precisely similar in all respects—not only the number of holes, but number of plants, number of second-sown plants, position of single plant holes, etc.

It is important that precautions should be taken to make certain of a good stand—by means of fine tilth, level ridges, pre-watering, delinting the seed, etc. But if a hole fails to germinate then re-sowing must be done in each of the three holes of the "triad," the germinated seedlings in the other two holes being pulled out. The triad is the name given to the three *corresponding* holes in the varietal ridge and the two control ridges. Fig. 1 shows permissible forms of triad, straight and oblique, into which holes can be grouped.

Fig. I



At thinning time, equalization is again obtained by cutting out plants, so that each hole of a triad contains the same number of plants. If one hole of a triad contains one plant only, the other two holes of the triad must be reduced to one plant. If a hole is blank, the other two holes must be pulled out. By making use of the oblique triad it is possible to avoid too much pulling out, which reduces the stand and decreases the comparability.

Having thus obtained comparable lines, flower counts are taken in due course; and the yields at the end of the season are compared. Daily flower counts would mean, at Shambat, more work than the staff could manage. The system adopted is one of



taking flower counts on three consecutive days at intervals of seven or eight days. The mean of each three-day count is plotted as a point on the general flowering curves. Examples of these are given. (Figs. II and III.)

Precautions require to be taken at picking time, to avoid cross picking.

The produce from each line is bagged and labelled, and, later, ginned separately. The yield of the variety (seed cotton and lint) can then be expressed as a percentage of the mean of the two control yields. The expression of yields as percentages of the controls enables comparisons to be made between varieties.

To obtain comparisons of boll weight, seed weight, lint index, ginning outturn, fibre length, etc., special random samples of 100 healthy bolls are picked and laboratory records made on these.

Finally, in order to provide a check, one or two triplets are laid down in which the control type is sown on the centre ridge and tested against itself.

A criticism of this layout which leaps to the eye, is that with long adjacent lines of different varieties, if there is a difference in root development in the varieties, this unequal root competition will obscure the yields obtained, giving an advantage to the variety with the better roots.

But this rather than being a disadvantage is an advantage. Good root development is a desirable character for a new variety to possess, and the fact that a new variety probably has vigorous roots is an important point in its favour when selecting, and *vice versa*.

One of the results of this root competition is that in some cases very large differences occur between control and variety.

It is desirable to know, therefore, what order of difference should be considered when making decisions as to which type should be discarded, which expedited, and so on. It is admittedly unfortunate that the results are not open to statistical analysis.

## PRELIMINARY TESTING OF NEW VARIETIES.

Some estimate of the degree of reliability can, however, be made from figures obtained when the control variety has been tested in triplets against itself.

In the original trial in 1928, four such triplets were grown using the same Sakel for "variety" and control. The "variety ridge" yields expressed as percentages of the mean of the control ridges were 98.2 per cent., 98.8 per cent., 94.1 per cent. and 100.2 per cent.

In the 1931-1933 experiments (including tests with American varieties) the figures for the check triplets were 101 per cent., 99 per cent., 90 per cent., 99 per cent., 99 per cent. and 116 per cent. The last figure is unduly high, but it was found on checking the layout that the division lines were appreciably closer to the control ridge than they should have been. The control ridges were thus at a disadvantage compared with the control ridge of the triplet. This undoubtedly accounts for some, at least, of the divergence.

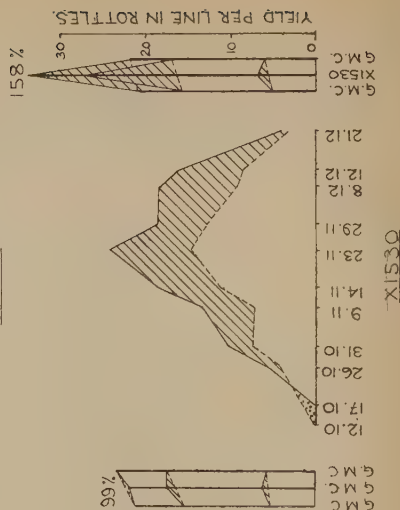
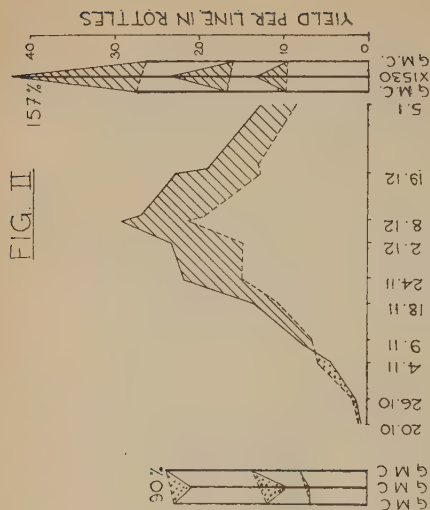
In the case of the 90 per cent. figure, this discrepancy cannot now be traced to anything definite, but in the detail of different pickings' weights, all the figures for the different pickings show good concordance except one, which is suspiciously low.

Further evidence was obtained in an experiment in 1932. Twenty-four ridges of Main Crop Sakel were considered as eight triplets and equalized just before first picking. Equalization at this late stage appears not to be as effective as when done in the beginning; but in spite of this, the maximum difference obtained was 14.7 per cent.

As a result of experience we have found that an increase of more than 50 per cent. justifies increased attention being given to a strain, and that 20 per cent. to 50 per cent. increases probably mean real increases in yielding capacity. A decreased yield of the variety of more than 20 per cent.—unless the variety has some other specially desirable characteristic—is held to justify the slowing down of work on a strain.

CONTROL VARIETY GEZIRA MAIN CROP SAKEL (G.M.C.) IN ALL  
 CASES FOR 1931/32 AND 1932/33 SEASONS  
 [SEE LEGEND ON FIG. III]

# 1931/32 SEASON



# 1932/33 SEASON

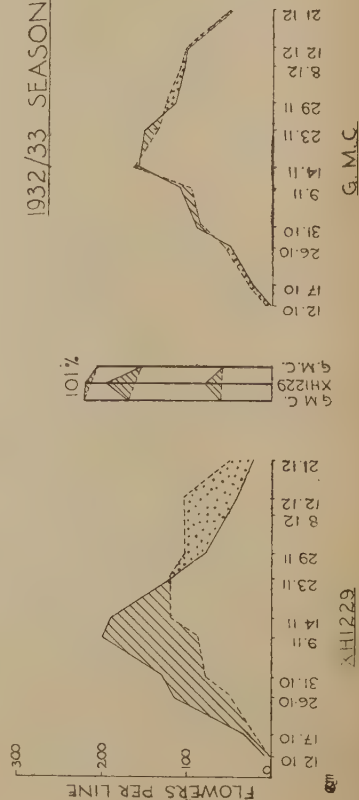
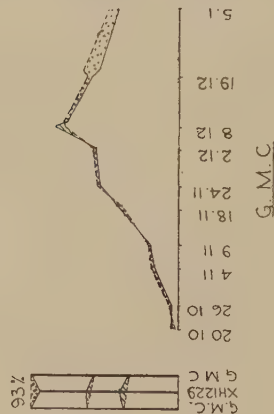


FIG. II

# PRELIMINARY TESTING OF NEW VARIETIES.

1933/34 SEASON.

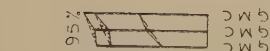
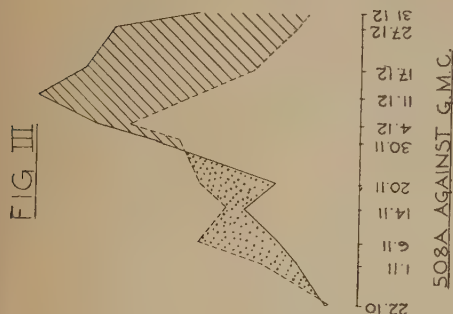
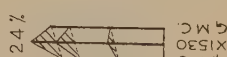
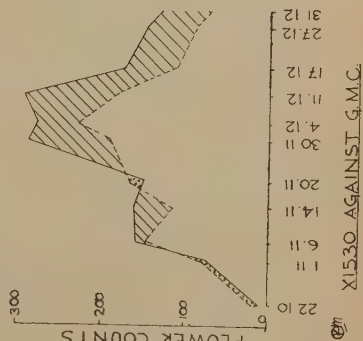
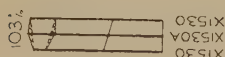
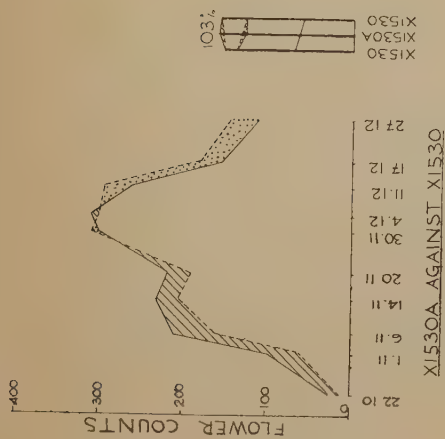
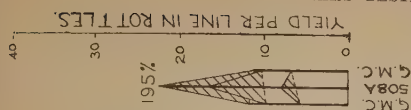


FIG III



## LEGEND FOR FIGS. II AND III.

(i) FLOWERING CURVES.

VARIETY

CONTROL

INCREASE OF VARIETY OVER CONTROL "HATCHED."

" " CONTROL " VARIETY "STIPPLED"

(ii) YIELD COLUMNS PLACED ON RIGHT OF CORRESPONDING FLOWER CURVE

"VARIETY RIDGE" YIELD

CUMULATIVE DECREASES OR INCREASES AT 1ST, MID- AND LAST PICKING SHOWN AS FOR FLOWERING CURVES

(iii) IN FIG. III THE VARIETY IS MENTIONED FIRST

ALL PERCENTAGES ARE IN TERMS OF CONTROL

[CONTROL = 100%].

In introducing his paper, MR. TROUGHT said that it dealt mainly with a point of technique for plant breeders. The "Line Test" as he called it, had been of great value to them in the Sudan in assisting in the selection of better types of cotton at a very early stage in their career. He could give instances where one year or even two years had been saved by the use of the method. It had been the subject of controversy with the statisticians, and he would be pleased to hear criticism of it or suggestions for its improvement.

#### DISCUSSION.

MR. HOSKING, referring to Fig. 1 of the paper, said that some of the triads shown did not seem to him to be permissible, owing to the difference in length between the plant in the control line and the plant in the variety line on either side.

MR. TROUGHT said that these should not be considered as being consecutive along the line, and gave an explanation of the particular case raised by Mr. Hosking. Judgment had to be exercised in connection with the general stand of the row. The triads that he had put down in his paper as permissible forms were found to work in practice.

PROFESSOR ENGLEADOW asked what would be the result if another control line were put in instead of the variety line in the middle of Fig. 1 in the paper.

MR. TROUGHT replied that that was one of their methods of getting a check on the reliability. He had shown in the diagrams the effect when a control line was substituted for a variety line. It would be noted that the flowering curves were very uniform and ran well together.

PROFESSOR ENGLEADOW said he thought the test was a satisfactory one. After all, the method did not set out to be of exceeding accuracy, but it was a working method. If Mr. Trought and his colleagues were satisfied that it passed that test, he would be prepared to say that it answered all the purposes required of it. He would not be prepared to say that it could be applied to other plants, but the method was intended for cotton, and any method that would test the productivity of strains in the early stages was a great advance.

MR. JACKSON raised a question regarding figures that were given in the paper illustrating the results of attempts to check the validity of the method. The greatest divergence was 16 per cent., and that was a special case. Later in the paper, however, it was stated that 20 per cent. to 50 per cent. increase probably meant a real increase in yielding capacity. He suggested that either the authors were unduly cautious in their latter statement, or the validity of the method when demonstrated



## PRELIMINARY TESTING OF NEW VARIETIES.

*by growing three rows of the same variety did not always work out as satisfactorily in practice as it did in the particular attempts which were made to prove it.*

MR. TROUGHT, in replying, said that he had been rather more cautious in the later paragraphs of the paper, bearing in mind, among other things, that root development did have an effect on the yields.

MR. PARNELL said that it was of great importance to be able to form some opinion as to the relative values of large numbers of strains in their early stages, and he asked whether this could be done in some way so as to give a test of statistical significance at the same time. To achieve this, a large number of repetitions would be necessary, and that would be against the method because it was intended for use with a small amount of seed and a small area of land. He suggested that, nevertheless, over a considerable number of tests of this sort the difference between the strain and the control might be worked out in the ordinary way, so as to give the statistical significance, and the results of a number of such tests used to assist the plant breeder in coming to a conclusion.

MR. TROUGHT in reply said that the question of finding a statistical basis for this type of experiment had been discussed with the authorities at Rothamsted. In their lay-out in the Sudan it was possible to divide up the line into eight portions, but in order to get any reliable statistical data, those eight portions would have to be scattered at random over all possible positions in the experiment.

PROFESSOR ROBERTS asked what number of varieties were handled in this manner; to which Mr. Trought replied that thirty or even more families were dealt with in one year, a fact that made their random scattering impossible, as the experiment would then have to cover an enormous area.

MR. LEWIN suggested that if, as Mr. Trought stated in his paper, he could not be satisfied with anything less than a 20 per cent. difference, neither statistics nor the lay out were of importance, as the difference would be apparent to the eye. He expressed surprise that Mr. Trought was ever able to get his ideal of 50 per cent. difference between the variety and the control. He (Mr. Lewin) had been in the habit of regarding a 10 per cent. increase, if it could be proved statistically to be a true difference, as a satisfactory advance.

MR. TROUGHT replied that he actually frequently did get a 50 per cent. difference, and it was the frequency with which such a difference had been obtained that led him to make the remarks contained in the last paragraph of the paper. In the Sudan there were large variations in the soil in quite small patches. By spreading the line over 100 metres

*or so, these small patches were to a certain extent levelled out, and the controls being adjacent there was a good chance that they would come under the same conditions.*

PROFESSOR HALDANE, *in summarising the discussion, said that if the method had the advantages claimed for it by the authors, it would clearly represent a considerable advance in technique, and he hoped that a large number of plant breeders would be willing to give it a trial.*

## COTTON BREEDING PROBLEMS IN NORTHERN NIGERIA.

BY

MEMBERS OF THE STAFF OF THE DEPARTMENT OF AGRICULTURE.

The following account of the work which has been and is still being done with regard to cotton breeding and selection in Northern Nigeria applies roughly to those provinces which grow American cotton and which are gazetted as American cotton areas, and in particular to Zaria Province, as the central experimental farm and the Botanical Laboratory are situated at Zaria.

2. Some two-thirds of the American cotton produced for export in Northern Nigeria is grown on the high plains, of which the town of Zaria is roughly the centre. This country is mostly 2,000 to 2,300 feet above sea level and extends 70 miles N.W. to Faskari and Mallamfashi, 20 miles North to Kudan, 100 miles South-Eastward to Rahama and 50 miles South to Kaduna. The rainfall and other data relevant to the climate of this area are given in the attached appendix. The other one-third of the crop is grown mainly in the Sokoto Province, but is not considered in this memorandum owing to lack of data. A good deal of cotton is also grown in Kano, but in recent years almost the whole of the crop from this area has been absorbed by the local weaving industry.

3. In the Zaria Province cotton is planted at various times from the middle of June till the middle of August, but July is the

## COTTON BREEDING IN NORTHERN NIGERIA.

usual time. In comparison with other cotton-growing countries the yield is always low; anything over 200 lbs. of seed cotton per acre is considered a satisfactory yield, anything over 400 lbs. is good, while yields of over 600 lbs. per acre are rare. It seems probable that the climate rather than the poverty of the soil is mainly responsible for this, for the effective flowering period is limited at the beginning by the excessive rain and cloud in August and September, and at the end by the increasing aridity of the soil and the atmosphere in November. If cotton is planted early enough to produce flower buds or flowers in August or September, such flowers are practically all shed. On the other hand, after the first week in November, very few flowers are produced at all and vegetative growth ceases. Thus cotton planted as late as the 15th August yields little owing to the drought in November, although it makes the most of the preceding months, and is seldom troubled by pests. Progressively earlier planted crops make correspondingly more vegetative growth, but the corresponding increase in yield soon reaches a limit. This limiting planting date seems to be between the 1st and the 15th July, for though June plantings produce more vegetative growth they are more severely attacked by pests (bollworms, jassids and even stainers), and usually yield a little less cotton than the July plantings. Since cotton planting must be delayed till July it is profitable to plant gero (Bulrush millet) on the cotton land with the first rains, so that it is harvested two to four weeks after the cotton has been planted.

Cotton planted in early July begins flowering towards the end of September. The days are then becoming warmer, drier and less cloudy, and the cotton plant enters on a period of intense activity in which the vegetative and reproductive functions are balanced. The flowering curve reaches a peak in the second half of October, and in the second week in November vegetative growth ceases altogether and flowering is much reduced. It is supposed that several factors contribute to this slowing down of activity, viz. : (1) the increasing number of ripening bolls; (2) the increasing difficulty of obtaining water from the soil (the rains having ceased in early October); (3) the increasing dryness of the air. During November the plants shed from a half to two-thirds of their leaves, but the remainder stay on for a long time. The period of maturation from open flower to open boll is about 53 days, so that the bolls begin to open at the end of November and the bulk of the crop is picked in December, very little being obtained in January and February.

4. From a consideration of the above it can be understood why even on good soils the yields in Zaria can never be as high as in more favoured countries, for the flowering period is limited by excessive rain in September and by drought in November; on the other hand the crop is harvested in excellent condition, because most of the pests are suppressed by the Harmattan and by the long dry season. On certain types of soil in Zaria it is almost impossible to get a yield at all, because either (i) the drainage is so bad that the plants are waterlogged during the heavy rains in August and September, and in consequence are stunted or die, or (ii) the soil dries out too quickly at the end of the rains so that the October flowering is suppressed, or in less severe cases bolls are formed, but open prematurely. The best yields are obtained on soils which do not dry out quickly at the end of the rains, but which are not so heavy or sticky as to become waterlogged in August.

5. Cotton can be grown in the Northern Districts represented by Northern Katsina, Northern Sokoto, Daura and Kano, which have a lower rainfall (25 inches to 35 inches), lower altitude (1,600 feet) and more sandy soil, but groundnuts are also successfully grown in these areas and, while the price was reasonable, this crop was grown for export to the exclusion of cotton. There, if cotton is to produce a crop, it must be planted with the first rains (in June). Considering one season's data from Daura (on the Northern Boundary of Nigeria) it seems that flowering was not suppressed in August, as the first picking from cotton, which was planted on the 13th June, was taken on the 13th October. Assuming the maturation period of the boll was 53 days as in Zaria, a considerable number of flowers must have been set by the 21st August. The 1933 rainfall of Samaru and Daura is given below:—

|                  | SAMARU.        |              | DAURA.         |              |
|------------------|----------------|--------------|----------------|--------------|
|                  | <i>Inches.</i> | <i>Days.</i> | <i>Inches.</i> | <i>Days.</i> |
| March ... ..     | —              | —            | —              | —            |
| April ... ..     | 0.97           | 5            | —              | —            |
| May ... ..       | 3.91           | 11           | 2.46           | 6            |
| June ... ..      | 9.14           | 17           | 5.09           | 9            |
| July ... ..      | 13.01          | 21           | 9.77           | 11           |
| August ... ..    | 13.75          | 24           | 11.84          | 17           |
| September ... .. | 10.44          | 19           | 3.14           | 11           |
| October ... ..   | 0.29           | 1            | —              | —            |
| November ... ..  | 0.12           | 1            | —              | —            |

The last picking at Daura was taken on December 7th, five days after the first picking was ready at Samaru (Zaria).

6. As has been indicated above, Zaria Province is the chief cotton-growing area and most of the selection work has been done in and for this area. The indigenous variety which used to be grown here was a form of *Gossypium peruvianum* (Hausa-*gwundi*), but in 1912 three exotic varieties were introduced and grown along with *gwundi* on the Government farm at Maigana near Zaria. These varieties were "Alien's Long Staple," "Nyasaland" and "Georgia." From these Allen was selected owing to its superiority in yield and quality. A system of pure seed supply was maintained with a basis of mass selection, and later of pure line selection, but there is little doubt that Allen became hybridised with the other cottons grown at Maigana before selection was started. A broker's description of the average cotton now grown is : "Strict good middling, Staple 1½." There is often a qualifying remark such as : "neppy," "wasty," "mixed with short," "soft" or "irregular."

7. Pure line selection in Allen was started in 1923 by making selections on our seed farm at Maigana and selfing the progenies. In 1928 the work was amplified, more plants were examined including many selections from native farms; the best strains were tested each year against bulk Allen obtained from the ginnery, and the lint from these trials sent to England for spinning tests. As a result of this work many strains were tried and discarded, and we are now left with one strain (L) which has given equal yield to bulk Allen in several yield trials at Samaru, and has produced lint valued 30 to 40 points higher than Allen, when the price of the latter was 5.55 pence and American Futures were 4.70 pence.

8. We are now experimenting with crossing with the aim of combining the general merit of L with the low immaturity of H.21 and with the jassid resistance of U.4/123. Some crosses have also been made between L and S.G.27. Strain L has not been found to suffer seriously from jassid attack, but it is attacked more than the U.4 strains.



9. Since Allen, Georgia and Nyasaland were introduced in 1912, thirty-five other varieties or strains have been introduced at various times, as follows :—

| <i>Year.</i> | <i>Variety.</i>            | <i>Source.</i> | <i>Remarks.</i>                                                                                                                              |
|--------------|----------------------------|----------------|----------------------------------------------------------------------------------------------------------------------------------------------|
| 1912         | Allen                      | Uganda         | Replaced the indigenous cotton in 1916.                                                                                                      |
|              | Georgia                    | U.S.A.         | D.*                                                                                                                                          |
|              | Nyasaland                  | Nyasaland      | D.                                                                                                                                           |
| 1921         | Hartsville                 | U.S.A.         | D., after yield trial and spinning test.                                                                                                     |
|              | Deltatype Webber           | U.S.A.         | D.                                                                                                                                           |
|              | Lighting Express...        | U.S.A.         | D. Very like Allen. No. 2 better.                                                                                                            |
| 1921         | Cokers Cleveland...        | U.S.A.         | D.                                                                                                                                           |
|              | Webber 49                  | U.S.A.         | D.                                                                                                                                           |
|              | Mexican Big Boll...        | U.S.A.         | D.                                                                                                                                           |
|              | Meade                      | U.S.A.         | D. Low yield.                                                                                                                                |
| 1927         | Ishan A.                   | S. Nigeria...  | Observation plots gave very low yields, but quality superior to Allen. D.                                                                    |
| 1928         | F.285                      | India          | Inferior to Allen in yield and quality. D.                                                                                                   |
|              | F.289                      | India          | Ditto.                                                                                                                                       |
|              | A.12                       | S. Africa      | Equal to Allen in yield and quality. D.                                                                                                      |
|              | U.4                        | S. Africa      | Yield trials in 1930, 1931 no significant difference from Allen. Brokers' reports and spinning tests have shown it barely equal to Allen. D. |
|              | Z.1                        | S. Africa      | Inferior in yield and quality. D.                                                                                                            |
|              | H.21                       | U.S.A.         | Superior quality to Allen (less nep). Inferior yield to Allen. D.                                                                            |
| 1929         | Acala                      | U.S.A.         | Succumbed to jassid.                                                                                                                         |
|              | Cawnpore                   | French Sudan   | Inferior in yield and quality. D.                                                                                                            |
|              | Cambodia                   | French Sudan   | Ditto.                                                                                                                                       |
|              | Mesowhite                  | Iraq           | Inferior in yield and quality (very neppy). D.                                                                                               |
| 1930         | Coimbatore 1               | India          | Inferior in quality. Equal in yield. D.                                                                                                      |
|              | " 2                        | India          |                                                                                                                                              |
|              | 3 strains of G. herbaceum. | India          |                                                                                                                                              |
| 1931         | U.4/123                    | S. Rhodesia    | Inferior in quality. Slightly inferior in yield. Still being tried (for crossing).                                                           |

\* Discarded.

## COTTON BREEDING IN NORTHERN NIGERIA.

| <i>Year.</i> | <i>Variety.</i> |         |     | <i>Source.</i> | <i>Remarks.</i>                                                                                                                                                                                        |
|--------------|-----------------|---------|-----|----------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| 1931         | ...             | U.4/64  | ... | S. Rhodesia    | Grown in small observation plots in 1931, but further investigation held up owing to lack of funds and greater promise of S.G.27 and S.G. 29 (two strains introduced at the same time). Not discarded. |
|              | ...             | U.4/4/2 | ... | S. Africa      |                                                                                                                                                                                                        |
|              | ...             | U.4/2   | ... | "              |                                                                                                                                                                                                        |
|              | ...             | H./77   | ... | "              |                                                                                                                                                                                                        |
|              | ...             | Z./14   | ... | "              |                                                                                                                                                                                                        |
|              | ...             | Z./9    | ... | "              |                                                                                                                                                                                                        |
|              | ...             | U./4/4  | ... | "              |                                                                                                                                                                                                        |
|              | ...             | C./15   | ... | "              |                                                                                                                                                                                                        |
|              | ...             | S.G.27  | ... | Uganda         | In one yield trial found nearly equal to Allen in yield. In spinning test definitely superior to Allen in quality. Still under trial.                                                                  |
|              | ...             | S.G.29  | ... | "              |                                                                                                                                                                                                        |

10. In 1934 we are going to introduce and try 195 strains produced by Dr. Harland.

11. The aim of all this work is, of course, the increase of profit per acre to the farmer by the supply of superior seed. The earlier part of this memorandum shows what has been done in this direction. Our problems now are: (1) We are not satisfied with the small superiority of L strain over Allen in Zaria Province, and hope to obtain increased superiority either by improving L by crossing or by selecting in Allen for increased yield, or by introducing superior strains such as S.G.27 or one of Harland's strains. (2) As a result of trials outside the Zaria area in 1933, there are strong indications that L is inferior to Allen in those parts. This is not surprising considering the differences of climate and soil. It is probable, therefore, that no single pure line will be satisfactory for the whole of the country, and in that case a separate seed multiplication scheme will be required for other areas. Zaria already has its seed multiplication scheme in which the E.C.G.C. farm at Daudawa is an important stage, the last stage in which the crop is still under complete control.

## APPENDIX.

## ZARIA CLIMATE.

The rainfall in Zaria township from 1904 to 1931 was as follows :—

| <i>Month.</i>    | <i>Average Rainfall.</i> | <i>Highest Figure for the month.</i> | <i>Lowest.</i>     | <i>Highest (1906) whole year.</i> | <i>Lowest (1907) whole year.</i> |
|------------------|--------------------------|--------------------------------------|--------------------|-----------------------------------|----------------------------------|
|                  | ins.                     | ins.                                 | ins.               | ins.                              | ins.                             |
| January ... ..   | ·002                     | 0·04                                 | nil (26 times)     | nil                               | nil                              |
| February ... ..  | ·018                     | 0·36                                 | nil (23 times)     | nil                               | nil                              |
| March ... ..     | ·81                      | 3·57                                 | nil                | nil                               | nil                              |
| April ... ..     | 1·75                     | 5·20                                 | nil (in 1904 only) | ·85                               | 2·20                             |
| May ... ..       | 4·61                     | 9·50                                 | 1·91               | 9·50                              | 3·69                             |
| June ... ..      | 6·46                     | 10·33                                | 2·37               | 5·95                              | 7·05                             |
| July ... ..      | 9·25                     | 15·11                                | 3·75               | 14·49                             | 3·75                             |
| August ... ..    | 12·35                    | 20·93                                | 4·46               | 16·39                             | 4·46                             |
| September ... .. | 8·48                     | 17·46                                | 3·63               | 9·90                              | 6·35                             |
| October ... ..   | 1·48                     | 4·36                                 | nil                | 3·95                              | 2·32                             |
| November ... ..  | ·024                     | 0·44                                 | nil                | 0·04                              | nil                              |
| December ... ..  | nil                      | nil                                  | nil                | nil                               | nil                              |
|                  | 44·73                    |                                      |                    | 61·07                             | 29·82                            |

Other details for temperature, evaporation, and relative humidity are available for three years (1929-1931) and are as follows :—

| <i>Month.</i>              | <i>Rainfall.</i> | <i>Daily Evaporation.</i> | <i>2 P.M. Relative Humidity.</i> | <i>Temperature.</i>     |                         |
|----------------------------|------------------|---------------------------|----------------------------------|-------------------------|-------------------------|
|                            |                  |                           |                                  | <i>Average Maximum.</i> | <i>Average Minimum.</i> |
|                            | ins.             | ccs.                      | °                                | °                       | °                       |
| January ... ..             | nil              | 92·3                      | 16·3                             | 90·1                    | 55·8                    |
| February ... ..            | nil              | 111·5                     | 13·6                             | 94·6                    | 62·1                    |
| March ... ..               | 9·17             | 111·5                     | 16·0                             | 101·1                   | 65·9                    |
| April ... ..               | 1·29             | 75·3                      | 31·3                             | 99·7                    | 70·3                    |
| May ... ..                 | 5·76             | 42·7                      | 49·3                             | 93·7                    | 70·1                    |
| June ... ..                | 5·87             | 26·9                      | 58·0                             | 88·2                    | 67·1                    |
| July ... ..                | 10·10            | 21·4                      | 65·0                             | 84·7                    | 66·8                    |
| August ... ..              | 12·85            | 18·0                      | 67·6                             | 84·1                    | 66·1                    |
| September ... ..           | 9·09             | 18·9                      | 63·0                             | 86·5                    | 66·5                    |
| October ... ..             | 0·92             | 36·6                      | 54·3                             | 89·8                    | 64·8                    |
| November ... ..            | 0·02             | 69·6                      | 24·0                             | 92·8                    | 60·7                    |
| December ... ..            | nil              | 88·0                      | 15·3                             | 91·4                    | 56·1                    |
|                            | 46·07            |                           |                                  |                         |                         |
| Highest single reading ... | 3·66             | 173·0                     | 97                               | 107·5                   | 75                      |
| Lowest single reading ...  | 0·01             | 1·7                       | 2                                | 70·0                    | 49                      |

The evaporation is that given by Livingston white spherical atmometers with the actual number of ccs. lost multiplied by the constant 0·81. The atmometers are porous spheres 5 cm. diameter with a neck for the water supply. They were exposed to the weather on a pillar 15 feet above the ground. The thermometers were housed in a small thatched shelter 7 feet high and mounted 2 feet below the thatch. The thermometer bulbs were level with the edge of the thatch. They were read at 7 a.m. and 2 p.m. (Nigerian time).

## COTTON BREEDING IN NORTHERN NIGERIA.

The figures for Relative Humidity were worked out from "Tables of the Reduction of Meteorological Observations" published by the Government of India in 1910. The lowest figure recorded was on March 22nd, 1930, when there was a strong Harmattan blowing. The thermometers stood as follows:—

| Dry bulb. | Wet bulb. | Difice. | R.H. |
|-----------|-----------|---------|------|
| 99.40° F. | 61.5° F.  | 87.9°   | 2    |

Relative Humidities as low as 5 are reached on several occasions every year.

MR. VOELCKER, in introducing this paper, regretted that no member of the staff of the Agricultural Department serving in Northern Nigeria was available. He thought that the paper was self-explanatory, and he hoped that the discussion on it would prove helpful.

## DISCUSSION.

MR. HOSKING expressed some surprise at the statement in the paper that it was profitable to grow bulrush millet on the same land with cotton. In Uganda they had tried planting cotton with groundnuts, and found that after the groundnuts were harvested the cotton produced only a very small crop. Under Uganda conditions the early stages of the cotton plant were very important.

He referred to the fact stated in the paper that the flowering period in Northern Nigeria was limited by excessive rain in September and bad drought in November, and he suggested that possibly the best variety in those circumstances would be an early maturing plant. Selections from Mr. Parnell's U.4/4/2. had given several cottons of that description.

MR. JACKSON referred to the point that Mr. Hosking had made with regard to the desirability of an early maturing type of cotton for the climatic conditions described. So far as he was familiar with the list of varieties stated in the paper to have been tried, those tested up to the year 1930 seemed to be distinctly late maturing varieties. He suggested that it might be profitable to try certain Indian types, on the ground that it might be more profitable to get a good crop of a somewhat inferior cotton than a risky crop of a better type.

MR. LEWIN replied to the questions that had been raised, on the ground that he had formerly served in the Department of Agriculture, Nigeria, though not in Northern Nigeria. He thought that it was undoubtedly profitable to grow bulrush millet with cotton. That conclusion was the outcome of a long series of very carefully costed experiments in the Northern Provinces, and the reason for it was that the native secures more profit from bulrush millet than he does from cotton. It was not contended that a certain proportion of the cotton crop was not lost, but the native got in addition a very valuable food crop.

## THE COTTON PROBLEMS OF KENYA.

BY

THE DEPARTMENT OF AGRICULTURE.

The cotton production of Kenya, though small, is of great importance locally, as it is the staple cash crop, actually or potentially, of considerable areas in the country. These areas may be grouped into three main regions, differing considerably in climatic and soil conditions: the Coast, the Lake region and the low-lying adjuncts of the Highlands on the North East (in the central Province). In no one of these regions is the crop important enough to warrant special seed selection work being done for the production of strains particularly suited to the local conditions, and Kenya must rely on the selection work being done in neighbouring territories where the production is much greater. This would, in principle, be quite a sound course to pursue as East Africa may be considered as one country for the purposes of investigation and research, and, generally speaking, similar conditions may be found in all the territories embraced in the term East Africa. Nevertheless, if such selected seed is to be imported into Kenya in bulk in any year, its quality will be likely to deteriorate after a period of years, and constant importations of fresh bulk supplies would be necessary. The cost of this may be considerable.

The general problem, apart from that of the actual type of cotton used, which is discussed below, resolves itself into a consideration of the desirability of leaving the whole of the selection and bulking work to neighbouring territories, and importing selected strains in bulk, as against the possibility of these neighbouring territories sending to Kenya seed of improved strains in the early stage of bulking for simultaneous multiplication. There are seed farms in the Kenya cotton areas where this work could be done.

2. With regard to the type of cotton to be grown, at present Kenya cotton is of the American Upland type. Egyptian cotton may be ruled out as unsuitable. Indian varieties have not been tried on the ground of their short staple; this objection is, however, not necessarily a valid one, and the views of the conference as to the relative merits of American and Indian types for differing conditions would be of considerable interest.

Of the American types, the Uganda strains are suited to the Nyanza area of Kenya, which borders on Uganda. On the Coast, and in the Central Province, however, it is open to doubt as to whether these strains will be the best suited. The Uganda cotton grown for some years at Malindi on the Coast, has deteriorated in



strength, though not in length, and this deterioration, while in part doubtless due to the absence of a cotton station where bulking of pure lines could be done, is possibly due mainly to the unsuitability of the type to local conditions.

A strain of U.4 has been tried at the Coast and the results indicate that this cotton is well suited to the coastal conditions and justify its introduction on a large scale. The conditions in Central Province are more allied to those of the Coast (in all save altitude) than of the Lake area, and it will probably be found that U.4 strains will be better suited to these conditions than the Uganda types ; both are under trial.

3. The experience of Officers at the Conference as to the rate of deterioration in cotton by use of the same seed, under varying climatic conditions, would be welcomed.

4. It is the experience in the cotton areas of Kenya, that it is a matter of great difficulty to establish the crop in a new district, permanently, in a reasonably short space of time. In any one area, even after the crop has been proved, constant and protracted propaganda is found to be necessary to ensure that it is absorbed permanently into local practice. With the unavoidable shortage of staff, the choice rests between concentrating on one area for a period of years until cotton is definitely established, and moving on progressively from location to location, introducing the crop, leaving the older areas to look after themselves. It is necessary for the economic welfare of many of the native areas to follow the latter course to get the crop known and cultivated as widely as possible, and the problem is to induce any body of natives in any area to take to the crop with intense propaganda for two or three seasons at the most, with the knowledge that they can then be left with confidence to keep on growing it, while other newer areas receive concentrated attention.

It is probable that one of the causes for this apparent lack of enthusiasm for cotton is the number of regulations which must be enforced to ensure the safety of the crop. These regulations, however, cannot be relaxed.

It would be of interest to learn the experience of workers in other developing countries as to the popularity of cotton and any means which are taken to ensure its rapid absorption into the agricultural practice of the native communities, always assuming a suitable place for it in the native agricultural economy.

A table is attached, showing the actual and potential production in the three main regions of the Colony.

SCHEDULE OF PROPOSED COTTON DEVELOPMENTS IN KENYA COLONY AND PROTECTORATE.

| Province.   | District.                        | Present<br>Production of seed<br>cotton. | Proposed Extension.                                                                                            | Population.    | Potential<br>Production of seed<br>cotton. | Notes.                                                                                                                                                                                                                          |
|-------------|----------------------------------|------------------------------------------|----------------------------------------------------------------------------------------------------------------|----------------|--------------------------------------------|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Nyanza ...  | North Kavirondo...               | 1932/33.<br>4,000,000 lbs.               | Natural extension in cotton areas<br>near Uganda border.                                                       | 60,000         | 20,000,000 lbs.                            | Main production is in N. & C. Kavirondo; this can be extended considerably both by increasing the area grown by individuals and by extending the cultivation to other locations. South Kavirondo is a new area to be opened up. |
|             | Central Kavirondo                |                                          | Cultivation to be developed<br>along Lake shore.                                                               | 230,000        |                                            |                                                                                                                                                                                                                                 |
|             | South Kavirondo...               |                                          | Very little grown at present;<br>cultivation to be developed to<br>justify erection of ginnery.                | 80,000         |                                            |                                                                                                                                                                                                                                 |
| Central ... | Kibos ...                        | Nil.                                     | An Indian settlement;<br>2,000 acres suitable.                                                                 | 83<br>farmers. | 550,000 lbs.                               | Ginnery lying idle at<br>Kibos.                                                                                                                                                                                                 |
|             | Fort Hall - South<br>Nyeri-Embu. | Nil.                                     | An area of 250 square miles in<br>the lower parts of Fort Hall,<br>S. Nyeri (Mwea) and Embu<br>(Upper Embere). | 60,000         | 3,500,000 lbs.                             | Cotton grown on very<br>small scale some years<br>ago. Suitable ginnery<br>site 10 miles from Sagana<br>Station. No other cash<br>crop.                                                                                         |
|             | Kitui ...                        | Nil.                                     | Area of 2,600 square miles<br>generally suitable.                                                              | 94,000         | 5,500,000 lbs.                             | No other cash crop.                                                                                                                                                                                                             |
|             | Machakos ...                     | Nil.                                     | ?                                                                                                              | ?              | ?                                          | Suitable cotton areas not<br>yet surveyed but known<br>to be considerable.                                                                                                                                                      |

## COTTON PROBLEMS OF KENYA.

| Coast | Lamu ...       | Very little, poor quality. | A coastal district—introduction of good seed from Malindi in 1934.                                | 10,000    | 550,000 lbs.                                       | Any cash crop will be a help to this backward area.                                                                                                                                       |
|-------|----------------|----------------------------|---------------------------------------------------------------------------------------------------|-----------|----------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
|       | Tana River ... | Nil.                       | See under "Notes."                                                                                | 8,000     | 300,000 lbs.                                       | Cotton development depends on the action taken with regard to the whole scheme of Tana River development. Whole Lower Tana River area would be suitable for cotton if and when developed. |
|       | Malindi ...    | 1,100,000 lbs.             | Natural extension through suitable parts of district.                                             | 28,000    | 2,000,000 lbs.                                     | Cotton established several years and rapidly growing in popularity. Ginners at Malindi.                                                                                                   |
|       | Kilifi ...     | First season of campaign.  | Natural extension through suitable parts of district.                                             | 69,000    | 4,000,000 lbs.                                     | First big production campaign now in progress and promising good results. New ginners at Mazeras.                                                                                         |
|       | Digo ...       | Nil.                       | Most of district suitable, especially Coast strip. Development to be undertaken following Kilifi. | 40,000    | 3,000,000 lbs.                                     | Served by Mazeras ginners.                                                                                                                                                                |
|       | Teita ...      | —                          | ?                                                                                                 | 15,000    | 1,000,000 lbs.                                     | Lower parts of district suitable.                                                                                                                                                         |
|       |                |                            |                                                                                                   | Total ... | 40,400,000 lbs.<br>= say, 30,000 bales of 400 lbs. |                                                                                                                                                                                           |

1st November, 1933.

The paper was introduced by MR. W. O. SUNMAN, who pointed out that the cotton problems of Kenya were more of an administrative than of a scientific character. They were those that are necessarily associated with a country of widely differing conditions, long distances, sparse population, and a limited staff. The officers working in the cotton areas were only able to devote a small portion of their time to the crop.

The present cotton production in the country was small, and its potential production was never likely to be very large. The Department consequently were not able to embark on any expensive programme of cotton selection work. One of the problems with which they were faced in consequence was at what stage to import their cotton seed ; that is to say, whether it was better to continue to import in bulk seed which had been found suitable in neighbouring territories under similar conditions, or whether to import seed of improved strains and do the bulking for themselves.

The other main administrative problem (apart from the question of the best varieties to be grown) was the reaction of the natives to cotton-growing. In Kenya the Bantu natives were not very keen on it, while the crop was more attractive to the Swahili natives on the coast. He would be very glad to hear the experience of officers in neighbouring territories on this point.

As regards cotton propaganda, the choice, as stated in the paper, lay between intensive propaganda in one district over a period of years, so as to get the crop firmly established among the natives in that district, or carrying on such propaganda for two or three years only in each district, and then transferring such activities to another area, hoping that the first area would not abandon the crop if the price dropped. The Agricultural Department would value any advice that the Conference might be able to give on these points.

#### DISCUSSION.

MR. SAMPSON said that he had just returned from a visit to Kenya, and the impression that he had derived was that the Department were trying to advance too quickly. In his view the foundations had not yet been laid for carrying on cotton propaganda among the natives. In the first place Kenya had no seed of its own. The seed was imported from Uganda, where the climate was very different from many parts of Kenya, and entirely different from that which is found in the coastal area. He felt that it was desirable in the first place to establish Stations where varieties could be worked up which were going to prove suitable in the locality in which the seed was to be distributed. He thought that the only area where Uganda seed might prove suitable was in the Northern Kavirondo district. In the coastal areas the climate might prove suitable to some types of Indian cotton.

## COTTON PROBLEMS OF KENYA.

*In the course of the subsequent discussion, it was suggested that the coastal area of Tanganyika was generally similar to that of Kenya, and that certain varieties of cotton grown in the former place might be found suitable. The necessity for close co-operation between the Agricultural Department and the Administration was also stressed.*

DR. TOTHILL suggested that it would be a mistake to establish new cotton breeding stations in Kenya if it were found possible to make use of existing stations in East Africa. He suggested that possibly a joint station might be set up for the coastal areas of both Kenya and Tanganyika, and that a station in Uganda might deal with the problems of the Northern Kavirondo. Many of the problems mentioned in the paper and in the discussion on it cropped up from time to time in Uganda, and Dr. Tothill said that the Agricultural Department in the latter Protectorate would welcome any representatives from Kenya who cared to pay them a visit, with a view to seeing to what extent the work that had been done in Uganda might be of assistance in the solution of the problems encountered in Kenya.

MR. TROUGHT asked whether the assistance of small Indian traders had been invited in encouraging natives to take up cotton-growing and to keep it going.

*In answer to the question in the paper as to the rate of deterioration in cotton by use of the same seed, Mr. Trought said according to his experience in India and the Sudan a commercial crop could be kept reasonably uniform for a number of years, if simple precautions were taken.*

MR. OATES replied that the small trader did not actually enter into the problem, because there was only a limited number of ginneries. These carried on propaganda themselves, and co-operated whole-heartedly with the Agricultural Department.

MR. MARSH asked how far the native had, as a rule, to carry his cotton to market, and Mr. Oates said in reply that the distance never exceeded ten miles.

DR. TURNER referred to the statement in the paper that cotton grown for some years in Malindi had deteriorated in strength, though not in length. He asked whether that deterioration was established as the result of a spinning test or from a broker's report, to which Mr. Oates replied that it was judged by a broker.

DR. TURNER urged the desirability of having spinning tests carried out before deciding as to any variation in strength of a cotton.



## CROP EXPERIMENTATION.

(Chairman : PROFESSOR F. L. ENGLETON.)

## PURITY TARGETS.

BY

C. G. HANSFORD AND J. D. JAMESON (*Uganda*).

This paper describes a development of the target diagrams described by C. H. Brown (*Empire Cotton Growing Review*, Vol. IX, 1932, p. 119). The method may be applied to any pair of factors in plant breeding, and in this instance has been used for the lint length and ginning out-turn per cent. of cotton.

Target diagrams have been constructed for certain of the more promising Progeny Rows, and each consists of observations from about 50 single plants. The diagrams have been drawn on transparent paper, with lint lengths as ordinates and ginning percentages as abscissæ, so that successive generations and related strains can be compared by superimposing their diagrams.

The ordinate representing mean lint length and the abscissa representing mean ginning percentage are drawn in as black lines. The problem has been to construct around the point of intersection of these two lines an ellipse representing the limit of variation (probability 20 to 1) of the strain. The method evolved is as follows :—

(a) The axes of the ellipse, which are drawn in as red lines are at an angle to the black lines representing the means. The value of this angle is obtained from the formula

$$\tan 2\theta = \frac{2r \cdot \sigma_g \cdot \sigma_1}{\sigma_g^2 - \sigma_1^2}$$

where  $r$  is the correlation coefficient of ginning percentage and lint length,

$\sigma_g$  is the standard deviation of ginning percentage, and  
 $\sigma_1$  is the standard deviation of lint length.

(G. Udny Yule: *An Introduction to the Theory of Statistics*, 7th Edn., 1924, p. 321.)

If  $\theta$  is positive, the angle is measured to the right of the vertical axis above the horizontal axis; if negative, to the left. This is a purely empirical finding. If there is no correlation  $\theta$  is 0 and the axes of the ellipse are vertical and horizontal. If  $\sigma_g = \sigma_1$  the ellipse becomes a circle and  $\theta$  is then 45°.

## PURITY TARGETS.

(b) The lengths of the axes of the ellipse depend upon converted values of the two standard deviations. These values are obtained by the two formulæ :—

$$\Sigma_g^2 + \Sigma_1^2 = \sigma_g^2 + \sigma_1^2$$

$$\text{and } \Sigma_g \Sigma_1 = \sigma_g \sigma_1 (1 - r^2)^{\frac{1}{2}}$$

(G. Udny Yule, *op. cit.*, p. 322.)

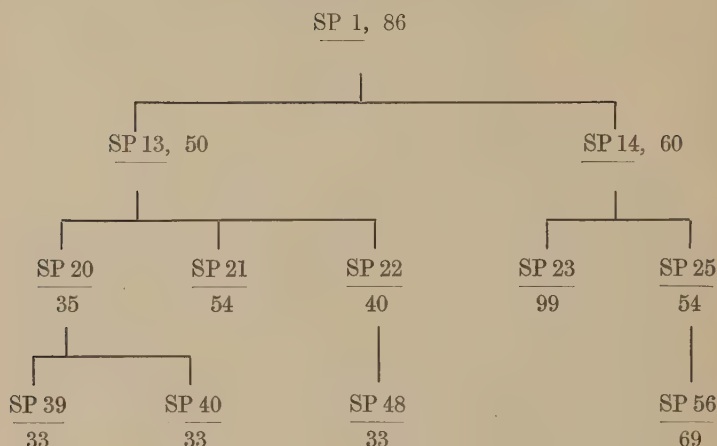
(c) The lengths of the semi-axes of the ellipse were at first taken to be  $t\Sigma_g$  and  $t\Sigma_1$ , the area of the ellipse then being  $t^2 \cdot \pi \cdot \Sigma_g \Sigma_1$ , where  $t$  is the P·05 value for an infinite number of degrees of freedom, *i.e.*, 1·96 approx., and  $t^2 = 3\cdot84$ . This value of  $t^2$  is that of the P·05 value of  $\chi^2$  for one degree of freedom. Mr. Yates of Rothamsted states that instead of this the value of  $\chi^2$  for two degrees of freedom should be used, so that the area of the ellipse is slightly increased. ( $\chi^2 = 5\cdot99$ .)

The circumference of the ellipse thus obtained defines the limit of variation (probability being 20 to 1) of the strain, as deduced from the sample used for observation. Plants lying outside this ellipse are definitely off type. Groups of plants near the centre of the ellipse are breeding true to type. Groups of plants within the ellipse in the right upper quadrant, of high lint length and high ginning percentage, show an improvement on the strain, and yet are not likely to split badly from type in respect to these characters; such groups are a sound basis for selection.

The variation for ginning percentage has usually been found to exceed that for lint length, so that the ellipse becomes elongated in the vertical direction. Moreover, the correlation between these two characters is usually negative, inclining the ellipse upwards to the left. Plants in the "selection quadrant" are therefore usually few in number, and when they do occur, tend to be outside the ellipse.

If the ellipse is found to diminish in area from generation to generation, provided the number of observation plants remains roughly the same, the strains are being purified in the characters used. Some of the areas obtained at Serere are quoted as a matter of interest (Area =  $\pi \cdot \Sigma_g \Sigma_1 \cdot \chi^2$ ).

## FAMILY U. 4/4/2.



MR. HOSKING explained that the subject of the paper started from some work that had been done in Uganda with a view to obtaining some data from the purity targets described by C. H. Brown. The paper showed the progress that had been made in attempts to obtain from the targets a figure representing the variation in two directions of any two characters. The figures for U.4/4/2 as re-selected in Uganda were rather striking. In Uganda they had found it quite easy to increase lint length and ginning out-turn separately within the strain, but much more difficult to increase them both together. However, one or two strains had shown positive increases in the right direction for both characters.

In the original targets on transparent paper lines were drawn through the ginning out-turn and lint length means, thus forming four quadrants. Selection in the right-hand top quadrant for three or four seasons had given an increase in both characters.

## DISCUSSION.

DR. BALLS said that extensions to the use of "targets" were continually being found, as Mr. Brown had described in the article quoted in the paper. But personally he regarded the method (which was, after all, merely the plotting of correlation diagrams) as a thing designed to evade statistical treatment and to substitute for it graphic inspection. He gave an example in support of the soundness of that point of view.

Suppose one had an  $F_2$  of a cross between two long characters and two short characters, giving four re-combinations. Plotting a diagram, one got four elliptical groups, each of them definable as individuals in the way suggested, and those four groups as a whole were not susceptible to any known statistical treatment. The whole thing could be treated as a correlation diagram, and a correlation worked out which was completely meaningless. In the extreme case, if correlation were perfect, all four groups would concentrate into four dots, yet "correlation" would be very low.

Again, suppose one had a rare segregate, such as they were getting in the work on hair properties which Mr. Brown was doing. It appeared as a little outlying group which in any formal treatment would be thrown out, though it was really the thing they were looking for. He thought it far better to leave the target method as an inspection method.

He believed that the biggest improvement in the technique was one he had recently introduced at Giza, of using logarithmic ordinates. In that case the area of the diagram was independent of the size of the character measured; and if the size were shifting from 30 to 40 millimetres one got exactly the same area of diagram as if it were shifting from 15 to 20 millimetres.

But to treat the target diagrams as anything more than pictures which helped the eye to realise complications in the situation was in his view a waste of technique.

DR. PEIRCE said that while he did not think target diagrams were particularly susceptible to very formal treatment, it was useful to have an objective method of defining an area which described a particular variety or type, for identification and so on.

One point he wished to bring out, which would make their interpretation much more definite, was that the diagram of all samples was really a complex of two different correlations—one of a particular strain with varied environment (a varied degree of maturity in particular), and the other of the several characters of different strains in a constant environment, in particular an environment which was amply favourable to the variety. It seemed to him that if the breeders in their characterisation of strains would concentrate on lint which was definitely mature, then two very different correlations would be separated in a way which at present, at the Shirley Institute, they could not attain. It would then be possible to determine separately the correlation between the characters of any particular uniform strain when they were given different environments.

DR. BALLS thought that Dr. Peirce was under a slight misapprehension as to what the cotton hair would do in the way of variation, even on a single seed. Their technique at Giza was to put down chequer plots in groups of 5 to 10 plants at the most. When they were

*ripening they were completely stripped of their bolls on a certain date, and then allowed to ripen bolls for another ten days. It was only the bolls which ripened during those ten days that were used for the purpose of plotting a target diagram. They used the shortest possible period, and would use one day's bolls only if enough bolls could be obtained in that time. But even material obtained in that way showed wide fluctuation; such fluctuation was quite inevitable.*

MR. HOSKING in reply said that he fully realised that there was nothing new in the paper; its intention was to seek information as to whether it was possible to find some simple arithmetical method which would give an area equivalent to the variation in two directions and to test its significance statistically. If this were possible, it would avoid the laborious task of plotting the target diagrams, and would place selection on a surer basis.

## THE OBSERVATION, MEASUREMENT AND RECORDING OF CROP LOSS AND CROP YIELD.

BY

MEMBERS OF THE CORPORATION'S STAFF IN SOUTHERN RHODESIA.

Continuous observation, measurement and recording of the crop, and of the different factors in the crop environment, are necessary for assessing the relative importance of the factors which cause crop loss. Furthermore, both observation and measurement are essential in the comparison of variables in the final result—yield of lint per unit area. A few of the difficulties affecting the experimental work at Gatooma, Southern Rhodesia, are outlined—especially difficulties in the statistical treatment of data on yield, crop loss, and factors affecting it. Laboratory measurements of crop quality characters are not dealt with.

### (A) CROP LOSS.

Loss of the potential crop takes place at the young squares, flower-bud stage, but this loss is not recorded at Gatooma. Plant developmental records on crops at Gatooma and elsewhere in the country show that flowering is adequate for the potential production of good yields of lint per acre. For convenience, therefore, crop loss is measured from the flowering stage of the plant's development.

*Shedding.*—Dated tickets are tied on the daily flowers, and the dates on which the bolls are shed, or picked as matured bolls, are recorded for each flower tagged. A record of shedding is thus obtained.



## RECORDING OF CROP LOSS IN SOUTHERN RHODESIA.

These data in conjunction with the data obtained by actual measurements of the daily flowering are used to calculate graphs of shedding.

The graphs of flowering and bolling, with the shedding records, give a fairly true picture of the periods of the making and the periods of loss of the crop.

The total loss due to shedding is analysed into (a) Bolls punctured by bollworm. (b) Bolls not punctured by bollworm.

(a) *Bolls punctured by Bollworms*.—The loss due to bollworms, as seen in the sheds, and at harvest in the matured bolls, is recorded by counts. Trained observation can generally differentiate between bolls attacked by American and Sudan bollworms, but in practice (when the work is done by Native recorders) a bollworm-punctured boll may be the result of the attacks of either American (*Heliothis obsoleta*), Sudan (*Diparopsis castanea*) or Spiney (*Erias* species) bollworms. Thus, supplementary to these records, in estimating the relative damage done by the different bollworms, observation must be made on their feeding habits and measurement made of their populations.

*Bollworm populations*.—For different crops, information is becoming available on the stage in the plant's development which is attractive to egg-laying by American bollworm moths. Therefore, when drawing samples for the egg and larval populations, due consideration must be given to the variables, age, and size of plant: and similarly with the Sudan bollworm moth. Measurements of the egg populations, of the larval instar groups, and of parasite populations, are made as fully and as accurately as facilities allow.

But with the bollworm populations so dependent on plant size and condition, there are considerable difficulties in attempting to make accurate records by sampling of populations; also in the comparison of populations by statistical methods.

When only relatively small samples of the plant population can be taken, the choice of the sample plants and sample areas becomes of great importance. Zones of higher and lower concentration of insects are to be found in the same field. For example, one recorder choosing plants and areas, which he knows to be attractive to American bollworm moths, could obtain counts 300 to 400 per cent. higher than a recorder choosing the plants and areas at random. The same difficulties apply to records of other insect populations, such as stainers.

Hence population records must be supported by much field observation and by supplementary counts in areas of plants differing in size and habit : and from the work already done, it would appear that the statistical treatment of such results bristles with difficulties.

(b) *Bolls not punctured by Bollworm.*—In this category are classified shed bolls which do not show visible signs of having been punctured by bollworms. The loss may be due to :—

- (1) Natural shedding—dropping of the excess of crop over that which the plant can carry through. This, of course, is considerably influenced by genetic differences between strains and by environmental factors such as soil conditions, droughts, excessive rainfall.
- (2) A weakening of the plant's vitality through jassid attack.
- (3) Loss through stainer damage.
- (4) Loss due to minor pests and diseases.

(1) Direct loss of bolls by a strain due to the interactions of genetical and environmental factors is masked by the effects of attacks by insect pests and disease.

In the field, strain differences due to natural shedding are estimated indirectly in the final yields, under a system of repeated plots for different localities. Thus the strains are observed over a season on a range of soils, and under varying conditions.

(2) Resistance to damage by jassid is strongly correlated with hairiness (a complex character) on the undersurfaces of the leaves. A measure of the insect nymphal population to be found on the leaf undersurfaces, in combination with hair counts, indicates the degree of resistance to damage by the insect. Strains are selected for the character hairiness, and so losses due to jassid attack are minimised.

(3) The four *Dysdercus* species, *fasciatus*, *intermedius*, and to a lesser extent, *superstitiosus* and *nigrofasciatus*, have been observed on cotton crops in the country. Continuous records of adult populations give data on the times of occurrence and the relative abundance of each species for any given season.

Visible damage by the insects, such as stained lint and punctures apparent in the walls of green bolls, are quantitative characters which bear some relation to the numbers of insects present in the crop.

## B.—CROP YIELD.

Weather conditions, attacks by insect pests—white ants, as well as the other pests mentioned—and the patchiness and irregularities of the soil make accurate yield trials (strain, spacing and fertiliser) difficult under the conditions that prevail at Gatooma and the out stations. Many of the differences between strains or other “treatments” can only be gauged by continuous observation—by “living with the crop,” not only on the Station, but at other centres throughout the country. Trials under as uniform conditions as possible are supplementary.

Actual measurement of differences, in terms of yield of lint per acre, is obtained by the use of Latin Squares, and Randomised Blocks plot technique; and the data are subsequently analysed by the Analysis of Variance. Whenever possible “treatments” number four or more; comparisons are made on the Latin Square arrangements of plots in preference to the Randomised Blocks arrangements. “Treatments” as a whole when compared with the error of the experiment are more significant under Latin Square arrangements and as a consequence the object of the experiment—to compare differences between individual treatments—is not as likely to be crippled as when Randomised Blocks arrangements are utilised.

The question arises, should not the Latin Square arrangement, given say, more than three variables, be generally used, in preference to the Randomised Block arrangement?

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In introducing the paper, which he said was written by Mr. McKinstry, a member of the Corporation's staff at Gatooma, Southern Rhodesia, MAJOR CAMERON said that it contained two chief points for discussion. Firstly they would like at Gatooma to have the views of the Conference as to whether in experimental work in which there were not more than three variables, the Latin Square should not be used in preference to the Randomised Block lay out. Secondly it was hoped in the paper to bring out the point that the variations that were encountered, both in the soil itself and in the intensity of insect attacks, often rendered ordinary plot technique as known in this country of comparatively little value. So many other factors interfered under Rhodesian conditions to obscure the final yield results. Observation, measurement and recording of the crop must be carried on all the time. Many of the differences between strains or other treatments could only be gauged by “living with the crop,” as Professor Engledow had first called it.

## DISCUSSION.

PROFESSOR ENGLEADOW suggested that the question of randomised blocks versus Latin squares should be discussed later on in connection with the paper on Analysis of Variance, where it more appropriately belonged.

MR. TROUGHT said he was pleased to see the statement in the paper that the yield of lint per unit area was the criterion of the final result in a yield trial. He said that in many cases the yield of seed cotton per unit area was taken for this purpose, but that was apt to be misleading as it took no account of the ginning out-turn of the cotton. It was the yield of lint that indicated the financial return to the grower.

PROFESSOR ENGLEADOW said it had become generally recognised that if one was either plant breeding or, by means of cultivation and other methods, endeavouring to raise the output or the monetary yield per unit area, then, from an experimental point of view, it was not sufficient merely to make treatments and to measure their final result. It was necessary to try to find out why one treatment was better than another; and the way to find that out was to study how the plant itself reacted at different stages of its life to the different treatments. That meant making various crop observations; for instance, boll shedding, flowering, etc., in the case of cotton.

He wondered whether some workers, among whom he would certainly be prepared to include himself, were not inclined to become rather doctrinaire in this matter, and to make these observations on plant development out of faithfulness to a sound doctrine, and whether all the observations that were made were fruitful. He believed that in some cases the statistical burden had become almost unbearable. He trusted he would not be considered to be speaking against statistical analysis. The advances which had been made in the last few years had brought statistical science to an extremely high level: but one had to recognise that the sheer labour of arithmetical calculation might be quite considerable. The volume of that labour was increased with the number of plant characters on which observations were made. The question on which he would be glad to hear the views of those with experience in cotton was whether there was or was not a general tendency to make more observations than, on the one hand, could be thoroughly examined statistically (because of shortage of labour in most cases), and, on the other hand, could be fruitfully considered in the local circumstances.

MR. BAILEY agreed with Professor Engledow that it was easy to waste time in taking superfluous observations. It was, however, a matter of great difficulty to decide in advance exactly what observations would in the end be found essential for the interpretation of the actual behaviour of the crop, and the safest course was therefore to take as many as circumstances allowed.

## RECORDING OF CROP LOSS IN SOUTHERN RHODESIA.

*He also drew attention to the difficulty of interpreting observations when they had been made, and, as an instance, took the case of "shedding" as a factor in crop loss. He quoted some recent results obtained by Mr. F. Crowther in the Sudan, where the removal of all buds and flowers from cotton plants on three occasions between early October and mid-November resulted in an actual increase in the final crop from 2.25 kantars to 2.48 kantars per feddan.*

MR. EVELYN said that he considered that observations in large quantities of different characters were the only means whereby a worker could become familiar with the strains with which he was working. His observations on shedding, budding and bolling were in agreement with those of the previous speaker, who said that initial bud shedding, rather than reducing yield, actually increased it.

*Another point of interest that emerged from those results was that the lower the node number, the greater the initial bud shedding, which apparently acted as a stimulus in elongating the main axis, increasing the number of sympodia or increasing the number of bolls per tree. Both shedding and number of bolls per tree appeared to be in some way related to the node number. In St. Vincent, in the days when the grower did his own selection, he always selected plants which shed little. Mr. Evelyn believed that by so doing he actually reduced his yield.*

## ANALYSIS OF VARIANCE AND ANALYSIS OF CO-VARIANCE, THEIR MEANING, AND THEIR APPLICATION IN CROP EXPERIMENTATION.

BY

J. WISHART (*School of Agriculture, Cambridge*).

Of recent years we have learnt how to conduct our crop experimentation according to certain well-defined scientific principles. Beginning in the home country, at Rothamsted, the new methods have spread until there must be few agricultural scientific workers in the Colonies who have not heard of the modern randomised lay-outs. The principles are simple and may be briefly re-stated here. If a number of treatments or varieties is to be tested, a number of repetitions or replications of each is insisted on, even if the unit plot area is thereby reduced. Technically, of course, the yield of a one-acre plot is equivalent to the average yield of the ten one-tenth-acre plots of which it may be regarded as composed. But this does not diminish the need for replication, which comes in when there are several "treatments" (this word will be used in what follows to designate the subjects of experimentation, whether they be fertiliser treatments, variety differences, cultivation processes, and so on). To each treatment



might be allotted one acre, say, of ground. Thus with four treatments we have four acres of ground covered, and if these are divided into forty one-tenth-acre plots we are set the problem of so arranging the plots as to deduce accurately figures for yield for the four treatments which shall be biased as little as possible on account of the varying fertility of the area in question. Ten plots chosen at random will obviously give by their mean a better expression of productivity over the area than one-quarter of the area, which will not be precisely similar to the other quarters. Not only so, but the variation round their mean of the ten yields will give a measure of the experimental error to be expected due to fertility differences and other chance causes. If the area is divided into ten blocks of land, each containing four one-tenth-acre plots, a still better expression of productivity will be obtained by choosing one plot at random out of each block and using the mean of the ten plots so obtained. This ensures that the total area is more satisfactorily sampled, although the variation of the ten yields will not now be a true expression of the experimental error to be expected between neighbouring plots within a block, owing to the pronounced fertility changes that usually occur over an area of this size. With a single series of ten plots there is no means of estimating this change apart from other components of error. But now suppose that ten plots are allotted to each of the four treatments to be tested, all treatments being represented in the block, and the arrangement being random subject to this one restriction. Then on averaging the replicates we have secured figures for the yields due to the four treatments freed reasonably well from any bias due to fertility changes. The variation of the block totals gives an expression of this, into which treatment differences do not enter, since all treatments are equally represented in each block. Equally the treatment means on being compared give an adequate expression of the effects sought, since each is a mean of ten plots, of which one occurs in each block. Statistical theory comes in in the working out of the results of the trial, for if the arithmetical analysis of the yield figures be arranged as in an analysis of variance, the block differences can be eliminated from the yield variations as an extraneous source of variation which has nothing to do with the differences to be compared, although unless the experiment was arranged in this way they would add considerably to the experimental error. The remaining variation is divided into two parts, that due to the treatments, and the remainder which cannot be attributed to either of the two sources, blocks or treatments, and which is therefore an expression of true error. The estimates of error from the two parts would be equal, within

the limits of random sampling, if the treatments had equivalent effects, but if we find from the appropriate statistical test that the treatment variation is so much greater than "error" that there is only a very small probability that so large a value could be obtained by chance, we are entitled to assume that significant effects are shown by the treatments tested, and an examination of the averages by means of a standard error calculated from the residual error terms shows what these effects are.

The method described is that known as "randomised blocks." It is obviously flexible enough to permit of testing any number of treatments with any desired degree of replication. Its efficiency is, however, limited. For practical reasons the number of plots in a single experiment cannot be too great. The most efficient arrangement is, therefore, where a relatively small number of treatments is being tested, so that the block is not large, and where reasonably adequate replication can be secured. Inadequate replication cannot be satisfactorily made up for by an increased number of treatments, for although the number of degrees of freedom available for the estimation of error may be of the same order as in an experiment with few treatments and many replications, that error itself will be larger owing to the increased size of the block, so that a large part of the fertility change over the area comes into error. The treatment means, owing to the smaller number of replications and the relatively large area of the blocks, will be less stable, and although we are guarded against unwarranted conclusions by virtue of the larger error, the whole arrangement is obviously less efficient. I know that there has been a tendency to proceed to complex experiments with two or more sets of interacting treatments, owing to the flexibility of the arrangement. The danger of loss of efficiency on this ground, and on the ground of inadequate replication, should be guarded against by every experimenter.

Another well-known method is that of the "Latin square." It must by now be familiar to you all, and I will content myself by saying that it applies where the number of replications is made equal to the number of treatments, the plots being arranged in a row-column system, so that each treatment occurs once in each row and once in each column of the square. Subject to these restrictions, the arrangement is random. It permits of a more effective elimination of fertility differences over the area, and is generally a very accurate method of experimentation.

When we have conducted the experiment to the best of our abilities and according to an approved method of randomised lay-out, we have done all we can with the area in question. When all is said and done, however, we are limited by the material we have to work with. It is obviously necessary to pay attention to the question of a suitable choice of site, and an area as uniform as possible should be selected. The previous history of the experimental area will be a guide in this respect. Now fertility, using the word in its most general sense, may often, to a large extent, be a permanent feature of the site, and a source of variation which appears in the experimental year as a part of error, and which, even with the best methods, cannot be reduced below a certain minimum value by means of the analysis of variance, may sometimes be capable of further reduction if information regarding the productivity of the same plots in previous seasons is available. This applies with special force to permanent crops. The yields of a plot for successive years may be positively correlated. If that is so, then it is possible to adapt the regression method to this particular problem. Briefly, this method "corrects" one variate, in this case the yield in the experimental year, for variation in the other, *i.e.*, the yield in a preliminary year; that is, it effectively finds out for us what the yields would have been in the experimental year had there been no systematic source of variation in the preliminary records. Not only so, but it measures the error remaining after such correction. Regression is an established statistical method, but it has to be modified when data such as the results of our agricultural experimentation are not homogeneous. The new statistical quantity involved when a second variate enters into our calculations is the co-variance, a measure of the co-variation of the two variables under consideration. When the data of each are amenable to an analysis of variance, then the co-variation can equally be examined by means of the technique of the analysis of co-variance. Effectively this enables us to eliminate the co-variation that is due to the gross fertility differences between the blocks of land, which may vitiate our estimate of plot-by-plot dependence, and so find the relationship between the variates when this source of heterogeneity has been removed. The justification for the application of the method lies in the reduction of error which may be the result, a reduction which must be sufficiently great to compensate for the preliminary experimentation which is necessary.

The method has not been used long enough to enable us to say how effective it can be, and I can do little more now than

suggest possible ways in which it may be of benefit. In doing so I will restrict myself to the cotton crop. It is conceivable that where an ideal site for experimentation is difficult to secure, information regarding the performance of the plots in a preliminary trial year may be sufficient to secure increased precision. In this connection it is not necessary that the plots in the previous year should be laid down to the same crop. Another possible application suggests itself. The yields of experimental plots may vary on account of unequal plant number, either through uneven germination or failure to survive, or for other reasons. This will undoubtedly be a more serious factor with a large plant like cotton, which takes up a considerable amount of ground, than with a plant like wheat, of which there may be several thousands on a plot. It is undoubtedly a common practice with experimenters to correct for stand, that is the yields of the plots are multiplied by a factor to make them equivalent to the yields of plots with exactly equal plant numbers. This may have the effect of reducing the error, but it brings in the incalculable factor that gaps in the plant population may stimulate the growth of adjoining plants, so that they yield more heavily than would be the case were the plants evenly spaced. Correction for stand may, therefore, not be as successful as the experimenter had hoped for. If the variation in plant number has seriously affected the yield, that should show up in an analysis of co-variance of yield and plant number, and in the existence of a significant residual regression. The further procedure is to work out a "corrected" analysis of variance of the residuals from the regression function, and test the resulting treatment effect for significance. This procedure may or may not have a marked effect in reducing the error. If it has, the experimenter is well rewarded for his statistical exertions, and the method has the merit that it deals with the experimental data exactly as they arise in the field, and not with data adjusted in a somewhat artificial way. The extra field labour consists in counting numbers of plants, a thing which would probably be done in any case.

The method of co-variance is described in the latest edition of R. A. Fisher's "Statistical Methods for Research Workers" (Oliver and Boyd), and those who have mastered the technique of analysis of variance as applied to crop experiments will have no difficulty in going through the further stages necessary in the statistical analysis when joint variation of two or more variates falls to be considered.

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PROFESSOR ENGLEADOW said that the papers by Mr. Wishart and Mr. Heath contained a great many side issues, but also certain major aspects which would perhaps be of the greatest general interest. He suggested, therefore, that the discussion on each paper should as far as possible be concentrated on the major aspects. He reminded the Conference that Major Cameron's question of the randomised block versus the Latin square had been carried forward to the paper about to be discussed.

MR. G. B. LAURENCE, in introducing Dr. Wishart's paper, said that in a short paper on a lengthy subject it was not possible to do more than state the principles underlying the methods. Beginning with a description of the "Randomised Block" layout, Dr. Wishart showed how by means of such a block system (1) the whole experimental area may be satisfactorily sampled by each treatment, and (2) advantage was taken of the correlation that was known to exist between plots close together, thus ensuring a comparatively uniform basis of comparison within each block or set of treatments. The analysis of variance was then the extremely convenient arithmetical process whereby the entire variation between all the plot yields might be split up into (a) the variation in yield between the blocks; (b) the variation between the treatments, and (c) the variation left over within the treatments after the gross block differences had been eliminated, this last being an expression of true error. With uniform treatments, it would not be expected that the between-treatment variance would differ from the within-treatment (or error) variance, hence the test of significance simply became the test of whether the between-treatment variance was significantly greater than the within-treatment variance.

Dr. Wishart sounded a note of warning against the inadequate replication that was sometimes adopted in very complex experiments. In such cases, the blocks were usually very large. A larger error resulted, and on the whole, the fewer replicates tended to make the treatment averages less stable and reliable.

The Latin Square was a logical extension of that method of eliminating soil fertility variation from error. Whereas in a randomised block the gross fertility differences between blocks might be eliminated from error, in the Latin Square a fertility variation might be eliminated in two directions at right angles.

The treatments were there arranged in rows and columns at random, with the one restriction that each treatment might not occur more than once in each row and each column. Thus the total yields of columns, say, might give an expression of the soil variation along the experimental site, and the totals of rows give the variation across the



site. The elimination of these variations in the A.V. table usually led to a considerably reduced error.

The analysis of co-variance had been introduced as a further extension of the efforts to increase the precision of field experiments, and its usage was based on the assumption that soil fertility may be an inherent characteristic of any experimental site. So, for example, in a preliminary trial on the site, we might obtain a series of values— $x$ , and in the succeeding actual experiment we might obtain a set of values— $y$ . We might then analyse the co-variation of these two things  $x$  and  $y$  on the same principle as the analysis of variance, eliminate the gross block and treatment relationships between  $x$  and  $y$ , and by difference obtain the residual regression relation between  $x$  and  $y$ . Should this be significant, there would be a very definite gain in precision by adjusting the experimental yields for the preliminary yields. It was a case of making use of inside information about the plot fertility variation supplied by the relation between preliminary and experimental yields.

The method had attained its greatest practical importance where, for some outside cause, an irregular stand had occurred. By then adjusting final yield for this inequality at the outset, by means of the analysis of co-variance, rather than by some artificial correction factor, it was possible to obtain the true treatment effect freed from the complication of the irregular stand.

Preliminary experiments often meant a great waste of time, because there was not a sufficient gain in accuracy, but the practical worker might possibly think of some method of taking supplementary observations which would give him information about his experimental area. Treatments applied after such observations could be tested with far more precision when the final yields had been corrected for those supplementary observations, provided that the relationship between supplementary observation and final yield was significant.

#### DISCUSSION.

MR. PARSONS asked if the supplementary observations to which Mr. Laurence had referred took place prior to the usage of an area for experimental plots, or concurrently with an experiment.

MR. LAURENCE replied that, as he had said, preliminary experiments were often a waste of time. They had found at Cambridge that a supplementary observation, such as a germination count, was often of considerable help to them, because from a final yield it was impossible to say whether it was the number of plants on the plot that had brought about that yield or whether it was the treatment that had been applied after that germination count. But having made that germination

count, and having recorded the final yields, then by getting the regression relationship between them, the final yield was corrected for that previous count, and, by the analysis of co-variance, it could be said what the treatment effect had been since that first count.

MR. F. H. GARNER said that for five years they had been trying the new methods of field experimentation at Cambridge. The types of problem with which they had been dealing were roughly as follows. By means of the Latin Square they had been studying the effect of nitrogenous manures on wheat yields; those manures had been applied in small quantities by hand, and for that purpose the Latin Square was an admirable method of experimentation. But in another type of experiment, the Spring cultivation of wheat, such as rolling and harrowing carried out in Spring, and also various methods of preparing Autumn seed beds, it was definitely much better to use the randomised block method of lay-out. Where horses and implements were used on the plots, there must be large discards; that was most inconvenient in a Latin Square, but with a randomised block lay-out the discards could be outside the experimental area.

Again, with the randomised block lay-out, if for some reason plots were seriously damaged at one end of the series, it was perfectly legitimate before starting to take observations, to omit whole blocks of plots. He suggested that that was an answer to Mr. McKinstry's problem, referred to in the paper from Southern Rhodesia, regarding some of the difficulties he had met with in experimentation. In his view that was definitely a point in favour of the randomised block lay-out, for if plots at one end were lost, the whole block could be ruled out; whereas in a Latin Square lay-out, if a plot or two were lost it was exceedingly difficult to know what one should do.

Passing on to another type of experiment, Mr. Garner said that they had been studying at Cambridge questions regarding variety and age of seed beans. In such an experiment, where different kinds of beans were drilled, the randomised block lay-out was much better than the Latin Square.

On grass-land, working largely with manurial problems, they usually applied from four to six treatments, and for that purpose either the Latin Square or the randomised block lay-out had been quite satisfactory.

If, on the other hand, there were less than four treatments, he thought without any hesitation that it was useless to consider the Latin Square, and one was forced to adopt the randomised block lay-out.

Finally, during the last 18 months they had been using the analysis of co-variance. It arose in the following way. They had been drilling an experiment with beans, and thought that the drill had been set to sow at the required rate per acre. The setting of the drill was

carried out with the drill jacked up at the homestead, and agitated. On the field, the drill set as planned did not perform in the same way as it had done at the homestead when jacked up. (The explanation was that the oscillations with the drill jacked up did not represent adequately the oscillations in the field.) The drilling of the beans was not quite in accordance with expectation, with the result that different seed rates had been used for the various varieties of beans. That was particularly unfortunate, because it was found at harvest that there was a fairly close association between the amount of beans sown and the yield on harvest, and it was only by using the co-variance method that it was possible to eliminate that difference of seed rate, and to draw definite conclusions, by assuming that varieties had been sown more or less at the proper amount per acre.

Another case in which the co-variance method was used was in an experiment in which drilling was being carried out on a seed bed that had been specially prepared with the Gyrotiller. On the Autumn (Gyrotiller) treatment was superimposed the Spring treatment of rolling, harrowing, etc. When the germination count was made significant differences were observed on the plots allocated for the Spring treatments, although these cultivations had not been carried out, and it was by using the co-variance method that it was possible to see the effect of those Spring treatments up to the end of the month of April when the last observations were made. He quoted that as a very good case to show the importance of that method in analysing data when things seemed to go wrong.

SIR GEOFFREY EVANS said that he wished to point out one particular danger which was arising, namely, the fact that so many of the experimental methods which were being evolved at the centres of higher learning and research were beyond the means of the ordinary Agricultural Department. For instance, at the Imperial College of Tropical Agriculture an experiment was being conducted on sugar cane. It covered about 20 acres, and the cost was between £200 and £300 a year. It required a very highly qualified man to run the experiment, and it required close supervision and very reliable labour for harvesting.

He wished therefore to put forward a plea to those engaged on these very important statistical methods not to forget the struggling Agricultural Departments, and to try, if they could, to give them some method which was feasible with the resources at the disposal of such Departments. Some of the suggestions which had been put forward were only applicable to such institutions as the Imperial College of Tropical Agriculture or Cambridge University. They were far beyond the means of an ordinary District Agricultural Officer in charge of a small station in the Colonies. He believed this point of view to be important, and it was one which he knew would meet with a good deal of agreement from officers who are working in tropical Agricultural Departments.

PROFESSOR ENGLEDOW said that he hoped the discussion would elicit opinion upon the practicability of carrying out statistically sound experiments, according to modern statistical views, in remote places.

From the applause following Sir Geoffrey Evans' speech, he had evidently touched a soft spot. Sir Geoffrey suggested that it was exceedingly difficult for the agricultural officer in the ordinary Agricultural Department in the tropics to carry out an elaborate experiment. It would be of very great interest if those who had experienced that difficulty would indicate its nature. Was it time? Was it rate of work? Was it complexity, or lack of highly trained assistants? Views on those points would be very helpful.

MR. PARNELL spoke of the difficulty often experienced at Cotton Experiment Stations of carrying an experiment through to the end. With advice from the statistician the initial lay-out was a simple matter, but such things as insects, hail, dust storms, wash, and so on could not be provided against.

On the question of Latin Squares versus randomised blocks, his view was similar to that of Mr. Garner, that randomised blocks were better in view of the fact that at Barberton he always had to discard some of the blocks, owing to such circumstances as he had just mentioned. Under their conditions it was possible to run a randomised block experiment, but they nearly always finished up with fewer blocks than the number with which they started.

In reply to the Chairman's question as to the principal factors which made it difficult for an agricultural officer in the Tropics to carry out an elaborate experiment, Mr. Parnell said that the chief cause was the large number of accidents that might occur to spoil the plots. He had enumerated several, and such things could not in practice be controlled.

MR. SUNMAN also dealt with the difficulties that beset an agricultural officer in carrying out experimental work. In his opinion the principal difficulty was shortage of efficient staff. A District Agricultural Officer might or might not have a Station near his residence, but he had to cover a very wide area in the course of his duties, which took him away from his Station for days and weeks at a time. He could lay out the experiment perhaps, but subsequently constant supervision was needed to give reliable results. In few places had he any trained European assistant, and there were not many trained natives who could be relied upon to look after an experiment in the absence of the Agricultural Officer. It was this difficulty that militated chiefly, in his view, against the success of any but the simplest experiment, that could be left with confidence whilst the Agricultural Officer went about his other duties.

MR. BAILEY put a question, which he suggested Mr. Yates might be able to answer, as to the proportion of cases in which preliminary uniformity



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trials, studied by the co-variance method, had actually succeeded in practice in reducing experimental error. He pointed out that, in a large number of cases, the fertility of the same piece of land might be expected to vary very considerably from year to year, owing to the position of the main working-zone of the roots varying with, say, rainfall or the height of an underground water-table.

MR. YATES.—At the Chairman's suggestion, Mr. Yates first dealt with a few points on the analysis of co-variance, before passing on to discuss complex experiments.

He first gave an answer to the question as to whether it paid to lay down a preliminary uniformity trial and use the data from this trial to increase the precision from the experimental results. He stated that in general a uniformity trial was well worth while, on grounds of increased efficiency, with perennial crops, but was definitely not worth while with annual crops. The point to bear in mind was that the amount of information from a one-year experiment would have to be at least doubled, i.e., the standard error would have to be reduced in the ratio of 1 to  $\sqrt{2}$ , if the preliminary trial was to justify itself.

He then went on to discuss a few general points in connection with co-variance. He pointed out that there was nothing magical about the process: it was merely the adaptation of ordinary partial regression methods of analysis to results from replicated experiments. The process in principle was simple: the observations themselves were used to indicate the magnitude of the effects of any subsidiary variable or variables, and if desired the results could then be corrected to allow for such effects. The correct use of the process depended on an understanding of the principles underlying the use of partial regressions.

There were three very different uses to which co-variance had been put in agricultural experiments. The first was that of finding the effect of some uncontrollable factor, such as insect attack. The second was that of studying the mechanism by which treatments produced their effects. The third, and that which had attracted most attention, was the increase in precision of experiments by the elimination of the effects of factors which could not be controlled, as for example, the initial weights of animals in a feeding trial, differences in fertility as indicated by a preliminary uniformity trial, differences in plant number from plot to plot. One vital point to be borne in mind in this last application was that the experimental treatments might themselves affect the variable eliminated; the result of any such effect would then be automatically eliminated from the final experimental results. In the case of plant number, for instance, certain treatments might reduce the number, and in such a case, the yields corrected for differences of plant number would benefit those treatments unfairly. Again, one could not utilise the first picking of a crop (after the treatments had been applied) to increase the precision of the general results, since



treatment effects (in so far as they were common to all pickings) would be entirely eliminated thereby. In the case of initial animal weights or uniformity trial data there was no danger of eliminating real treatment effects, since the treatments could not affect these quantities.

Mr. Yates then went on to discuss the objects and applications of complex experiments.

He stated that complex experiments were introduced in order to give increased efficiency and also to provide a wider inductive basis for whatever conclusions were drawn. Both objects were effected by using all combinations of several different sets of treatments, or of different treatments and varieties. If, for instance, it was desired to find out the effect of varying dressings of nitrogen and also the differences between three varieties, then, instead of putting down two separate experiments, one on varying levels (say 0, 1 and 2) of nitrogen, and the other an experiment on the three varieties, a single experiment of double the size containing all combinations of the nitrogen treatments and varieties, i.e., 9 treatments in all, could be made. Information was then obtained not only on the effect of nitrogen and the difference between varieties, but also on the question whether different varieties responded differently to nitrogen, or (alternatively stated) whether variation in the amount of nitrogen affected the differences between varieties. (This last type of effect was denoted by the term *interaction*). Dr. Wishart in the paper under consideration had criticised such experiments on the ground that there was loss of efficiency because of inadequate replication. Clearly there was loss of accuracy (but not of efficiency) on the comparison of any particular pair of treatments as compared with the accuracy that would be obtained if the whole experiment were devoted to this particular pair of treatments. But this was not the sort of comparison that was useful; what was required was the average varietal differences and the average nitrogen effects, and also some assurance that these varietal differences were the same, whatever the level of nitrogen (within limits) in the soil, and that the response to nitrogen was the same, whatever the varieties. In the complex experiment the varietal differences and nitrogen effects would be determined with an accuracy twice that obtained from the two simple experiments, provided that the standard error per plot was the same, and, in addition, information would be obtained on the existence or otherwise of interactions.

If interactions did not exist, then the experimenter might justifiably regard the average varietal differences obtained as representative, irrespective of the level of nitrogen manuring, and equally the average nitrogen responses he might regard as representative, irrespective of which variety of the three was sown, or (reasoning inductively and regarding the three varieties as a sample from all varieties ordinarily grown) of any variety likely to be sown. If on the other hand interactions were found to exist, then information would presumably be

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required on the response of each variety to nitrogen, and it might be judged that the experiment did not furnish sufficiently precise information on this point. In that case further experimentation would be necessary, but nothing would be lost, since the information already obtained could be combined with that obtained later.

The other ground on which Dr. Wishart had criticised complex experiments was that the blocks became so large that the differences of soil fertility were inadequately eliminated, and the standard error per plot was thereby seriously increased. This was a valid criticism, though how far the resultant loss of efficiency counterbalanced the gain in efficiency due to complex arrangements had never been closely investigated; it was known, however, that the loss of efficiency from this cause was not usually very great. Moreover, a further device had been introduced in order to enable the block size to be kept small while having a large number of different treatments: namely that of confounding certain unimportant treatment comparisons (usually high order interactions) with block differences.

Other objections to complex experiments were that their complexity rendered them difficult to lay down, peculiarly liable to accidental mistakes in the field, and difficult to interpret. The weight of these objections depended on circumstances, and each experimenter had to decide for himself whether complex experiments were of use to him or not. But it was the speaker's opinion, from his own experience of survey work in the Gold Coast, that difficulties of utilising native labour in the tropics to carry out complex operations were much exaggerated. Difficulties of this nature tended to decrease very rapidly also, as experience was gained and a trained personnel was built up.

MR. SAWHNEY asked whether, by the use of analysis of co-variance, inequalities of germination or the effect of differences in the number of plants could be eliminated.

MR. YATES replied that the method would deal with such a point. The co-variance worked out, from the observations themselves, how much an additional plant on the plot would yield. It was nearly always less than the mean yield per plant, owing to competition, but the co-variance method automatically took that into account and was the only sound method of adjusting plant number.

MR. JACKSON confirmed what other speakers had said as to the difficulties arising from non-preventable occurrences. On one occasion at Indore they had laid down a complex experiment after taking advice from Rothamsted, but not a single area could be harvested because they were all swept away by wash.

In the past three years, they had attempted to introduce at Indore Randomised Block and Latin Square experiments, first of all on the experimental farm itself, and subsequently in outside districts. The latter had necessarily to be under the direct control of insufficiently trained or entirely untrained staff. They did not expect that this

*extension work would be successful at first, and regarded it rather as a useful training for the staff, but he had been agreeably surprised at the amount of valuable information they had obtained in this way. They had prepared a series of standardised forms and questionnaires on which records should be taken or information given, with spaces on the forms for the answers to the questions, and they had found as a fact that they had in this way obtained a moderately satisfactory proportion of the data required. In his view the principal factor was not so much the knowledge and training of the individual in charge, but his character. If he were conscientious, and faithfully followed the instructions issued to him, the desired information was usually forthcoming. On some occasions a man who was almost illiterate had handed in excellent results, owing to his having taken pains and having done what he was asked to do. Mr. Jackson said that in his experience the difficulty of conducting randomised block experiments in out-stations might easily be exaggerated. As one became more familiar with them, and as those actually in charge became more familiar, much of the difficulty disappeared. He admitted that at first it looked an alarming and complicated business, but once one had got over the state of stage fright, it was usually found not to be so bad after all.*

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## **SAMPLING AND GROWTH OBSERVATIONS IN PLANT DEVELOPMENT STUDIES ON COTTON.**

BY

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Plant development studies carried out on field experiments are of importance from three points of view. They provide data for the elucidation of the growth-weather relation. They give information as to the effects of the treatments on the physiological processes of the plant which result in the final yield. They are also what might be termed an insurance policy against the possibility of failure to obtain yield figures from the experiment. If, for example, the experiment is so damaged by insect attack that yield figures are unobtainable, the plant development records obtained earlier in the season will give some information as to the effects of the different treatments.

The scheme of plant sampling methods and growth observations described in this paper is being tried at Barberton on field experiments of cotton. It is the outcome of the suggestions made by Dr. E. J. Maskell at the last Conference, and of the example set by Dr. Gregory and Messrs. Crowther and Lambert in the Sudan. The scheme is still in its preliminary stages and is no doubt capable of being improved in many respects. It is therefore offered as a basis for discussion.

## SAMPLING METHODS.

All the experiments are of the randomised block type and are hand-planted with accurate spacing. The sample plants, either for fresh and dry weights, etc., or for observation in the field, are found by methods essentially similar to that used by Wishart and Clapham [1] for potatoes. On every plot two (or more) "sampling-units," located independently and at random, are taken on each occasion. Each sampling-unit consists of a number of plants arranged systematically with respect to one another so as to give a good "scatter" over the plot. This is attained as follows: If there are  $N$  plants in the area of plot to be sampled (after allowing for guard rows and edge plants) and a sampling-unit is to consist of  $n$  plants, a random number from 1 to  $\frac{N}{n}$  is taken as the "starting-point."

After the first plant corresponding to this number, every  $\frac{N}{n}$ <sup>th</sup> plant is taken. In practice this merely means counting plants to find the starting-point. After this it is only necessary to move across a certain number of rows and then up (or down) so many plants for each subsequent sample plant. The random starting-points for the sampling-units on each plot are obtained from Tippett's Tables [2] and inserted on a plan of the experiment.

If this method is used, certain combinations of number of plants per sampling-unit and size and shape of plot are liable to give sample plants arranged in a single line across the plot. In such cases, if there are  $R$  rows each of  $P$  plants in the plot, a random starting-point is taken in the first  $\frac{R}{n}$  rows (*i.e.*, in the first  $\frac{RP}{n}$  plants). After this, one takes a plant in each  $\frac{R}{n}$ <sup>th</sup> row, moving forward  $\frac{2P}{n}$  plants in that row and if necessary continuing to count plants up from the other end of the same row. In this way it is ensured that the sample plants shall be in at least two lines across the plot. If  $\frac{R}{n}$  is not a whole number, it is necessary to leave out one or more rows of the plot. The row or rows to be left out are then chosen at random for each sampling-unit separately, thus giving every plant in the plot an equal chance of being taken.

If the spacing were irregular so that plant number per plot varied, it would be possible to take *e.g.* every 20th plant for an approximately 5 per cent. sampling-unit, giving a varying number of plants. This was done by Wishart and Clapham. An alternative would be to work in yards of row length and count paces instead of plants. The nearest plant to the last pace would then be taken.



The sample plants, whether for weights or field observation, are taken strictly according to the above sampling methods with the following exceptions: if the number falls on a gap, a "topped" or broken plant, or a plant next to a gap, the nearest suitable plant is taken instead. In addition, before the thinning has been carried out, sample plants are only taken from holes with two or more seedlings. If the hole to be sampled has only one seedling, a similar sized plant is taken from a neighbouring hole. In this way depletion of the stand before thinning is avoided. When the number falls on a hole with two or more seedlings, the largest one is taken, *i.e.* the plant that would normally be left by a boy in thinning.

The field experiments on which these sampling methods are being used fall into two categories:—I. Experiments having plots large enough to allow of sub-division; part of the plot being used for regular plant sampling throughout the season and part being retained with its stand unimpaired for final yields. II. Experiments in which the stand must not be depleted, either because the plots are too small to sub-divide as above, or because of the effect on the subsequent year's crop. The schemes for these two types of experiment are outlined below.

*I. Experiments regularly sampled for weights, etc., throughout season.*

Each plot is divided transversely into three parts, the middle part being retained for final yields and the two end portions being used for sampling. For comparing the results of sampling with final yields it would be preferable to have one of the three sub-divisions of the plot allotted to yield at random. This, however, would tend to increase the "plot" error both for sampling data and yield data considered separately. For purposes of the sampling method the two end portions of the plot are treated as if they are contiguous and form a single plot.

Plant samples are taken about a week after germination date (as determined by daily counts), and then fortnightly throughout the season. Care is taken that half of the plants in each sampling-unit come from one end portion of the plot and half from the other. The following measurements, etc., are made on the sample plants, all the plants in a sampling-unit being treated together:—

Total fresh weight.

Fresh weight of cotyledons + leaves (this has to be omitted when plants have become large).

Number of leaves, squares + flowers, green bolls, open bolls, nodes on main axis.

Length of main axis.

Dry weight of cotyledons + leaves, squares + flowers, green bolls, seed-cotton, open boll carpels, stem.



## SAMPLING AND GROWTH OBSERVATIONS.

*II Experiments which cannot be regularly sampled for weights, etc., throughout the season.*

In this case the whole plot is used for samples and records. Plant samples are taken about a week from germination date, and then weekly up to the time of thinning. The following measurements, etc., are made on each sampling-unit :—

Total fresh weight.

Fresh weight of cotyledons + leaves (while plants are small).

Number of leaves, squares, nodes on main axis.

Length of hypocotyl and total length of main axis.

Dry weight of cotyledons + leaves, squares, stem.

After thinning, plants are selected each week exactly as if for sampling, but instead of being cut they are measured in the field. The number of nodes on the main axis is counted and the height above the cotyledonary node measured. (In working up the height data, the length of hypocotyl obtained for each plot from the samples before thinning is added to the heights obtained on that plot after thinning. In this way the effect of the changes in ground level through the season, due to cultivation, etc., is eliminated.)

Both before and after thinning, the number of plants in each sampling-unit which have reached certain stages of development is noted. These stages are, first leaf expanded, first sympodial branch showing an expanded leaf, first square, first flower, first boll open. The date, found by interpolation if necessary, when 50 per cent. of the plants have reached one of these stages is taken as the date of that stage. In this way the development on each plot can be expressed as time relations of successive major stages, as has been done at Tashkent [3].

If possible, one or two further samplings for fresh and dry weight are made at important stages in the plant's development, *e.g.* at time of first flowering and at first boll opening. The depletion of stand due to such occasional samplings is in most cases unimportant.

The advantages of taking a fresh sample of measurement plants each week are twofold. In the first place, it is more satisfactory from a statistical point of view, since the results for one week may be confirmed by those for another.\* This is especially important when, as is often the case, the size of sample is somewhat inadequate (*see below*). In the second place, it avoids the frequent handling of the same plants, which has been observed to affect the growth of the plant at Barberton and elsewhere.

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\* This applies to the study of treatment effects and general trends of growth. On the other hand, when plants are being measured every day or every two days, in order to study the relations between short period fluctuations in meteorological conditions and fluctuations in growth rate, it seems preferable to retain the same plants throughout. This is done at Barberton.

The above scheme (II) gives data on height, node number and dates of "stages" throughout the season on a statistically satisfactory basis, and also yields important information as to fresh and dry weights up to thinning date and at one or two important stages later. Since up to thinning date the plants are comparatively small, it is then possible to sample two or three different experiments for weights on different days of the week. This could not be carried on regularly throughout the season owing to the large amount of work involved when the plants are big, apart altogether from the question of depletion of stand. It seems probable that those Stations not equipped with drying apparatus could do fresh weights instead of dry weights.

Flower counts have been purposely omitted from the schemes, as the daily trampling of the ground and handling of the plants have been found to have a very marked effect upon growth. In both schemes I and II the number of nodes gives a measure of rate of differentiation of new parts, while in the case of I a substitute for flower counts is also provided by "number of squares + flowers."

#### METHODS OF DRYING, WEIGHING, ETC.

The sample plants are cut off at ground level and immediately placed in tins. (Later in the season they are cut up and placed in large billy cans.) After being fresh-weighed in the tins the plants are dissected into leaves, squares, bolls and stems; the three former items being counted as they are removed. The main stems are measured and their nodes counted.

One of the difficulties in plant sample work is the drying of the large quantity of material obtained. This has been largely overcome by the use of "drying-cupboards." The plant material is first killed in a hot water oven at about 95° C. (the stems having been previously cut into short lengths and the green bolls cut open). It is then spread out on wire gauze trays which slide in one above the other to form the shelves of a series of cupboards. The cupboards are open top and bottom and are heated underneath by "Perfection" blue-flame oil stoves. A sheet of tin on each stove spreads the heat, and the current of warm air passing up inside the cupboard rapidly dries the plant material. This is left in the drying cupboards until it appears thoroughly dry, and is then given a final drying in the hot water oven at 95° C. for 6 to 8 hours before weighing. In the early part of the season when the weights are small, all dry weights (and fresh weight of leaves) are done on spring balances of the Joly type. Later a quick acting beam balance weighing to 0.05 grms. is quite sufficiently accurate.

#### SIZE OF SAMPLE, AND SAMPLING ERRORS.

The size of sample is obviously limited by the number of sample plants that can be dealt with at a time. This may be determined by

drying space. Staff is also an important factor at Barberton since all weighing, measuring, node counting, etc., must be done by Europeans, the natives being unsuitable for any work involving judgment. If the experiment is to be sampled fortnightly for weights throughout the season, the size of sample after thinning date cannot exceed 2-3 per cent. or the stand will be depleted too rapidly.

The accompanying table shows some examples of sampling errors, etc., obtained in five experiments, which include two different seasons and two different soil types. The two Cultivation Experiments were sampled as in Scheme I and the others as in Scheme II. The results for height of main axis (shown in italics) and total fresh or dry weight on the same occasion were obtained from the same sample plants and are, therefore, directly comparable. Column 2 of the table shows the actual area sampled per plot, excluding guard rows and end plants. Column 3 shows age of plants in days from germination. It may be mentioned that the "first leaf," "first square" and "first flower" stages occur at approximately 8, 38 and 66 days respectively. In column 5 are shown the number ( $n_s$ ) of sampling-units per plot  $\times$  number ( $n$ ) of plants per sampling-unit, and the size of the whole sample per cent. of a full stand. The sizes of sample tried range from 1.4 per cent. to 7.1 per cent. The number of degrees of freedom available for estimating the sampling error is shown in column 6. The magnitude of the sampling error  $\sigma_s$  (for the mean of  $n_s$  sampling-units) is shown in column 7, where it is given as a percentage of the general plot mean,  $m$ . The loss of accuracy due to sampling errors can be judged from column 10, which shows the sampling variance  $\sigma_s^2$  as a percentage of the observed plot variance  $\sigma_p^2$  (*i.e.* an estimate\* of the percentage of the observed plot variance due to sampling variation). A comparison of the figures in columns 8 and 9 also indicates this loss of accuracy. Column 8 shows the estimate\* of what the true plot error (per cent. of the mean) would be if there were no error in finding the plot data, while column 9 shows the observed plot error  $\sigma_p$  (also per cent. of the mean) which includes the error due to sampling. The magnitude of the effect of the experimental treatments is shown in column 11, which gives the relative variance due to treatment, and the significance of that effect in columns 12 and 13.

The plants in the examples given for 1932-33 were distinctly irregular owing to early damage by a dust storm and hail. This is reflected in the high sampling errors obtained. The plants on the Rotation Experiment 1933-34 (loam soil) were also irregular, partly owing to uneven germination, and the sampling errors are higher than the corresponding figures on the granite soil.

\* These estimates are subject to considerable errors, being based on the difference between two variances.

It will be seen that both for total weight and height the percentage of the observed plot variance estimated as due to sampling variation is very high in practically every case, indicating that the sampling is inadequate. Almost the only exception is the 7.1 per cent. sample taken on the Cultivation Experiment 1933-34 at 36 days. This is about as large a sample (60 sampling-units of 8 plants each) as could be dealt with under present conditions, even so early in the season. After thinning it would become quite impossible for weights, both on account of size of plants and depletion of stand. The generally high level of the sampling errors for weight, and the consequent high observed plot errors, means that only comparatively large treatment effects can be detected. It will be noted, however, that in nearly all the examples in the table the effect of treatment on weight is significant in spite of the high observed plot errors. Thus information as to treatment effect is being obtained, even with the admittedly inadequate sampling. Furthermore, if the interaction of treatment and occasion is insignificant for two adjacent sampling dates, the results of the two samplings may be combined. This gives, as a rule, a considerable reduction in the observed plot error. For example, combining the 64- and 78-day dry weight data for the Cultivation Experiment 1933-34, reduces the observed plot error from about 21 per cent. to 13.9 per cent. and increases the value of  $z$  for treatment effect from 1.16 (64 days) and .75 (78 days) to 1.45. (Logarithms of the dry weights were used, as the plot relative variances on the two occasions were approximately the same.)

In the case of the Rotation Experiments the relatively constant values obtained for the sampling error (either for weight or height) after the 15-17 day samples, indicate that it is estimated with reasonable accuracy, *i.e.* with a sufficient number of degrees of freedom. For similar experiments next season it is, therefore, intended to increase the size of sampling-unit as much as is practicable, but to continue to take only two sampling-units per plot. For the samples from next season's Cultivation Experiment, however, it is hoped to take 3 sampling-units, each of 4 plants after thinning, from every plot. This will give a 2.7 per cent. sample and  $15(3-1) = 30$  degrees of freedom for estimating the sampling error.

The sampling and plot errors for weight found at the initial sampling (8-10 days), and at 15-17 days in the Rotation Experiments (not shown), are very much lower than those found later on. A curious feature, however, is that in all three experiments for 1933-34 the samples at 22-24 days still show much lower observed plot errors, although the sampling errors are now practically the same size as those found later. Thus the sampling errors increase considerably between 15-17 days and 22-24 days, while the observed plot errors

do not do so until after 22-24 days. This results in absurdly low estimates of the true plot error at 22-24 days. (In the case of the Cultivation Experiment the sampling and plot errors at 36 days have also been calculated as for a 3.6 per cent. sample in order to make them comparable with those at 22 days. In calculating the "observed" plot error and percentage of "observed" plot variance due to sampling it has been assumed that the "true" plot error would be that found for the 7.1 per cent. sample.)

The sampling and true plot errors for height are much lower than those for weight in almost every case, showing that the uncontrolled variation both within plots and between plots is less in terms of height than in terms of weight. Since the relative variance due to treatment is also consistently lower for height, the plant's response to treatment is also less in terms of height than in terms of weight. This might be expected, since the uncontrolled variation and variation due to treatment are both mainly aspects of the plant's response to differences of soil environment.\* It will be seen from the table, however, that in nearly all the cases investigated the *significance* of the treatment effect (as shown by the value found for  $z$ ) is also lower for height, *i.e.* the variation due to treatment is smaller in comparison with the observed plot variance. This indicates that at least in the range of experiments tried, height forms a less satisfactory measure of the plant's response to treatment than weight.

It was hoped to give some other results bearing on the relative values of various single measures (*e.g.*, height, leaf weight, etc.), in estimating general growth and development, but the data are not yet fully worked up. Perhaps members of the Conference will have information to give on this point.

I wish to thank Dr. E. J. Maskell for advice on some of the statistical methods used.

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\* The uncontrolled variation includes the inherent variability of the plant, variation due to natural soil differences and that due to differences in incidence of insect attack or climatic factors. In the case of the sampling variation it also includes errors of weighing or measurement. There is no doubt, however, that in most cases the natural soil differences both within plots and between plots are the main factor.



| 1.<br><i>Experiment.</i>                                                                                                                 | 2.<br><i>Area Sampled.</i> | 3.<br><i>Days.</i>                                                        | 4.<br><i>Measure.</i>                                                                                                                                                                      | 5.<br><i>No. Plants<br/>and Size of<br/>Sample per cent.</i>                                                                                                                                                                                                 | 6.<br><i>D.F. for<br/>Sampling.<br/>Error.</i>                                   |
|------------------------------------------------------------------------------------------------------------------------------------------|----------------------------|---------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|----------------------------------------------------------------------------------|
| Fertilizer Exp. 1932-33.<br>5 treatments $\times$ 6 blocks.<br>3' 6" $\times$ 2' spacing.<br>Loam soil.                                  | 1/24 acre                  | 55-6<br>"                                                                 | Fresh wt.<br><i>Height</i>                                                                                                                                                                 | 2 $\times$ 6 $\equiv$ 4.6%<br>"                                                                                                                                                                                                                              | 30<br>"                                                                          |
| Cultivation Exp. 1932-33.<br>3 treatments $\times$ 4 blocks.<br>3' 6" $\times$ 2' spacing.<br>Loam soil.                                 | 1/29 + 1/29<br>acre.       | 49<br>"<br>63<br>"                                                        | Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i>                                                                                                                                       | 2 $\times$ 3 $\equiv$ 1.4%<br>"<br>2 $\times$ 3 $\equiv$ 1.4%<br>"                                                                                                                                                                                           | 12<br>"<br>12<br>"                                                               |
| Cultivation Exp. 1933-34.<br>3 treatments $\times$ 5 blocks.<br>3' 6" $\times$ 2' spacing.<br>Loam soil.                                 | 1/28 + 1/28<br>acre.       | 8<br>22<br>36<br>"<br>"<br>64<br>"<br>"<br>78<br>"<br>92<br>"<br>106<br>" | Dry wt.<br>Dry wt.<br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i> | 2 $\times$ 8 $\equiv$ 3.6%<br>2 $\times$ 8 $\equiv$ 3.6%<br>4 $\times$ 8 $\equiv$ 7.1%<br>"<br>2 $\times$ 8 $\equiv$ 3.6%<br>2 $\times$ 4 $\equiv$ 1.8%<br>"<br>2 $\times$ 4 $\equiv$ 1.8%<br>"<br>2 4 $\equiv$ 1.8%<br>"<br>2 $\times$ 4 $\equiv$ 1.8%<br>" | 15<br>15<br>45<br>"<br>15<br>15<br>"<br>15<br>"<br>15<br>"<br>15<br>"<br>15<br>" |
| Rotation Exp. 1933-34.<br>Cotton after rotation crops.<br>6 treatments $\times$ 6 blocks.<br>3' 6" $\times$ 2' spacing.<br>Loam soil.    | 1/18 acre                  | 8<br>22<br>"<br>36<br>"<br>43<br>"                                        | Dry wt.<br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i>                                                                                                | 2 $\times$ 5 $\equiv$ 2.9%<br>2 $\times$ 5 $\equiv$ 2.9%<br>"<br>2 $\times$ 5 $\equiv$ 2.9%<br>"<br>2 $\times$ 5 $\equiv$ 2.9%<br>"                                                                                                                          | 36<br>36<br>"<br>36<br>"<br>36<br>"                                              |
| Rotation Exp. 1933-34.<br>Cotton after rotation crops.<br>6 treatments $\times$ 4 blocks.<br>3' $\times$ 1' 6" spacing.<br>Granite soil. | 1/24 acre                  | 10<br>24<br>"<br>31<br>"<br>38<br>"<br>66<br>"<br>87                      | Dry wt.<br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i><br>Dry wt.<br><i>Height</i>                                        | 2 $\times$ 6 $\equiv$ 2.9%<br>2 $\times$ 6 $\equiv$ 2.9%<br>"<br>2 $\times$ 6 $\equiv$ 2.9%<br>"<br>2 $\times$ 6 $\equiv$ 2.9%<br>"<br>2 $\times$ 6 $\equiv$ 2.9%<br>"<br>2 $\times$ 6 $\equiv$ 2.9%                                                         | 24<br>24<br>"<br>24<br>"<br>24<br>"<br>24<br>"<br>24<br>"                        |

\* Calculated from 7.1% sample,

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| 7.<br>Sampling<br>Error<br>per cent.<br><br>$\frac{100}{m} \sigma_s$                           | 8.<br>Estimated True<br>Plot Error<br>per cent.<br><br>$\frac{100}{m} \sqrt{\sigma_p^2 - \sigma_s^2}$ | 9.<br>Observed<br>Plot Error<br>per cent.<br><br>$\frac{100}{m} \sigma_p$                       | 10.<br>Estimated<br>per cent. of<br>Observed Plot<br>Variance due<br>to Sampling.<br><br>$\frac{100\sigma_s^2}{\sigma_p^2}$         | 11.<br>Relative<br>Variance due<br>to Treatment.<br><br>$\frac{V_t}{m^2}$                                     | 12. 13.<br>Significance of<br>Treatment Effect.                                                               |                                                                                                                                                                   |
|------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------|---------------------------------------------------------------------------------------------------------------|---------------------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------------------------------------------|
|                                                                                                |                                                                                                       |                                                                                                 |                                                                                                                                     |                                                                                                               | P                                                                                                             | z found ; z reqd.                                                                                                                                                 |
| 17.6<br>6.6                                                                                    | 5.4<br>6.0                                                                                            | 18.4<br>8.9                                                                                     | 91.3<br>54.5                                                                                                                        | 0.182<br>0.022                                                                                                | < .01<br>> .05                                                                                                | .84 ; .74<br>.50 ; .53                                                                                                                                            |
| 21.7<br>10.1<br>22.2<br>10.3                                                                   | 4.0<br>—<br>21.7<br>7.0                                                                               | 22.0<br>8.7<br>31.0<br>12.5                                                                     | 97.1<br>$\sigma_s^2 > \sigma_p^2$<br>51.2<br>68.7                                                                                   | 0.354<br>0.041<br>0.091<br>0.014                                                                              | < .05<br>< .05<br>> .05<br>> .05                                                                              | 1.00 ; .82<br>.85 ; .82<br>— ; .82<br>— ; .82                                                                                                                     |
| 5.3<br>10.6<br>7.3<br>2.7<br>10.4*<br>16.6<br>7.0<br>18.2<br>7.1<br>13.4<br>3.6<br>11.5<br>4.3 | 6.6<br>1.4<br>14.5<br>5.9<br>14.5<br>11.6<br>4.0<br>12.8<br>5.5<br>15.9<br>6.2<br>21.5<br>6.7         | 8.4<br>10.7<br>16.3<br>6.5<br>17.8*<br>20.2<br>8.1<br>22.3<br>9.0<br>20.8<br>7.2<br>24.3<br>8.0 | 39.2<br>98.3<br>20.2<br>17.6<br>33.7*<br>67.3<br>75.2<br>67.1<br>62.0<br>41.6<br>24.8<br>22.2<br>29.2                               | 0.006<br>0.106<br>0.171<br>0.020<br>—<br>0.417<br>0.104<br>0.221<br>0.021<br>0.231<br>0.034<br>0.059<br>0.013 | > .05<br>< .01<br>< .05<br>< .05<br>—<br>< .01<br>< .01<br>< .05<br>> .05<br>< .05<br>< .05<br>< .05<br>> .05 | — ; .75<br>1.12 ; 1.08<br>.93 ; .75<br>.78 ; .75<br>—<br>1.16 ; 1.08<br>1.38 ; 1.08<br>.75 ; .75<br>.48 ; .75<br>.84 ; .75<br>.94 ; .75<br>.43 ; .75<br>.37 ; .75 |
| 5.7<br>12.3<br>7.1<br>15.4<br>6.9<br>14.1<br>6.4                                               | 9.0<br>1.7<br>4.0<br>21.9<br>8.0<br>21.0<br>8.4                                                       | 10.7<br>12.5<br>8.1<br>26.8<br>10.6<br>25.3<br>10.6                                             | 28.7<br>98.1<br>75.7<br>33.2<br>42.8<br>31.3<br>37.1                                                                                | 0.043<br>0.168<br>0.024<br>0.456<br>0.049<br>0.458<br>0.054                                                   | < .05<br>< .01<br>< .05<br>< .01<br>< .01<br>< .01<br>< .01                                                   | .66 ; .48<br>1.19 ; .67<br>.64 ; .48<br>.93 ; .67<br>.74 ; .67<br>.99 ; .67<br>.78 ; .67                                                                          |
| 7.5<br>10.8<br>5.5<br>10.4<br>4.2<br>9.0<br>4.3<br>4.3<br>4.4                                  | —<br>—<br>—<br>12.4<br>6.3<br>11.2<br>6.2<br>5.4<br>6.0                                               | 6.2<br>8.7<br>5.3<br>16.2<br>7.6<br>14.3<br>7.5<br>6.9<br>7.5                                   | $\sigma_s^2 > \sigma_p^2$<br>$\sigma_s^2 > \sigma_p^2$<br>$\sigma_s^2 > \sigma_p^2$<br>41.3<br>31.3<br>39.2<br>32.4<br>38.7<br>35.4 | 0.011<br>0.051<br>0.016<br>0.046<br>0.003<br>0.086<br>0.009<br>0.020<br>0.013                                 | < .05<br>< .01<br>< .01<br>> .05<br>> .05<br>< .05<br>> .05<br>< .05<br>> .05                                 | .54 ; .53<br>.95 ; .76<br>.88 ; .76<br>.28 ; .53<br>.17 ; .53<br>.72 ; .53<br>.24 ; .53<br>.71 ; .53<br>.43 ; .53                                                 |

on that true plot error the same.

PROFESSOR ENGLEADOW said that the paper upon sampling and growth observations was closely connected with the previous one. He thought two points at any rate would be of greatest interest : one was the practicability of the sampling technique as ordinarily practised, and the possibility of dealing with a sufficient number of samples to give the degree of accuracy of result that was required ; and the second point was whether, with our present methods and with the present staffs of our experiment stations, there was a probability that a thorough experimental study of such things as the effects of different kinds of cultivation could be made. In other words, were present methods and resources adequate to the difficulties that experimentalists had to face ?

In introducing this paper, MR. PARNELL said that he was somewhat at a disadvantage, because he had not had an opportunity to read it carefully. It was unfortunately not ready at the time when he left South Africa.

Work such as that described in the paper was in their opinion at Barberton very desirable, owing to the fact which he had already mentioned that only too frequently experiments failed to give satisfactory results when regarded from the point of view of final yield, owing to their having been damaged by insects or weather, or some other cause. It would consequently be very desirable if a method could be found for ascertaining what were the effects of different treatments on the plant during its growth. As an instance he mentioned that they had observed a very definite effect of a previous fallow on cotton crops ; they had observed this over a number of years, and it was particularly obvious in the early stages of the crop. At the end of the season, however, there would possibly not be a very big difference in the final yield, and without something in the nature of an analysis of the type described in the paper, it was difficult to record such an effect. Other instances were provided by the effect of various manurial treatments, which, though visible in the course of the season, sometimes did not show up in the final yield. At times this was doubtless the correct result of the experiment, but at other times the crop might have suffered severely from insects, for example, which would entirely mask the effect of the treatment. The object of the work, therefore, was two-fold : firstly, to follow the effect of different treatments on a plant, and secondly, to get some result from the experiment in cases in which this would not be provided by the final yield. One chief difficulty was that of dealing with sufficient quantities and sufficient numbers of samples to enable the method to be treated satisfactorily from a statistical standpoint. At the same time, he thought the method useful, and it was intended to follow it up at Barberton. He hoped the

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method would provide some indication of general plant development : possibly no one series of measurements would provide this satisfactorily, but it ought to be possible to discover which measurements were best suited for the purpose, to concentrate upon those, and thus reduce the work so that a larger number of plants could be taken in samples to satisfy the requirements of statistical treatment.

## DISCUSSION.

DR. SANDERS said that the type of problems in connection with sampling that confronted them at Cambridge were not quite the same, apparently, as arises in the case of cotton. The method of drawing samples of trees described in the paper would be very suitable in the case of root crops, but that method had not so far been used at all at Cambridge. Their problems had usually related to cereal crops, and although different considerations arose, perhaps their experience in that connection might be of some interest.

The great trouble with the cereal crops was the necessity of walking on the plots, which upset the treatment very considerably. Taking random samples over an area almost completely failed for all practical purposes, and so they had for a year or two adopted a method that they knew to be bad at the time, namely restricting the samples to the ends of the plots ; yard lengths at the ends of the plots were sampled, though it was realised that the sampling area was thus a separate thing from the experimental plots. That method, however, proved useless, and subsequently the plots were normally made one drill breadth wide, and bridges were constructed which would just cross that distance ; foot lengths of row were sampled at random over the plots, and they were reached by lying at full length on the bridges. Precautions were taken to ensure that an equal number of samples was taken from each coulter of the drill. Occasionally coulters of drills went wrong : consequently, if there were eight coulters in the drill to be sampled, say, 24 foot lengths of samples were taken, and the only restriction laid down was that each coulter length should include three ; subject to that condition, samples were taken all over the plot, which ensured that the plot was adequately covered. In other cases, with beans, for example, their practice was to divide the plot into four quarters and take two samples at random out of each quarter. Using these methods it had been found that the sampling error was reasonably small.

A much more difficult crop was grass land ; that crop hardly came within the sphere of this Conference, but he might perhaps say in connection with it that it was necessary to know not only the green weight cut, but also the dry weight obtained, and a separate sample must be drawn from each plot if the analysis was to be applied in the end.

MR. P. E. TURNER, speaking with reference to cultivation experiments, said that they had found it possible in Trinidad to carry out such experiments under plantation conditions on sugar estates, and experience had shown that the results obtained could be introduced with confidence into field practice.

*The experiments were laid out in the form of randomised blocks. The plots were long and narrow to suit the conditions—20 to 30 feet wide and perhaps some hundreds of feet long. A minimum of seven replicates of each treatment were included. A strip through the experimental area at right angles to the length of each plot was reaped. All the experiments were carefully costed and recommendations were made on the net financial gain to the acre.*

*It had been found useful to include manurial as well as cultivation treatments in cultivation experiments, e.g., in any one experiment three types of cultivation might be compared, each with and without a nitrogenous manure.*

*Some of the results obtained from cultivation experiments were not in accordance with established practice and might lead to important changes in field methods ; fundamental changes in manurial policy had also resulted from the manurial experiments.*

DR. PHILLIS said that on the Cotton Station in Trinidad they used a minimum sample of 50 for leaves and bolls, and by doing so had found that the duplicate samples agreed within 2 per cent. or 3 per cent. Where whole plants were sampled, they graded carefully on height and node number and diameter of the stem at the base. By that method it was possible to use samples of ten plants and to ensure an accuracy of 2 per cent. between samples. Dr. Phillis suggested that similar methods might be useful at Barberton, and he believed they would be found to give better results than the methods of random sampling.

DR. BALLS said that the last speaker's suggestion was substantially identical with a method which had been adopted by a good many workers with cotton ; in that crop there was the advantage of having continuous records of flowering and bolling, from which the "average plant" could be defined on any day of the season. He had used that as the base-line when sampling. His practice was to take the sample plants and see what their condition was, how many flowers they had produced or were producing, how many bolls, note their height, and correct their condition to that of the "average plant" as recorded in the plant development curve. That method gave a very considerable reduction in the sampling error.

MR. PARNELL reverted to the matter of the damage that was likely to be caused to a plant when it was continuously under observation for record purposes. In the ordinary way, there was danger of such a



plant not being a normal sample of the crop at all, and he asked for the opinion of members of the Conference as to how it was possible to keep records of a plant and at the same time not damage it so badly that it was quite different from other plants that had not been touched.

DR. BALLS said that in Egypt they had the same problem to face. Messrs. Bailey and Trought investigated it some years ago, and introduced a system, which was still in use, of duckboards for reducing the error due to treading on the soil. That did not obviate damage to the plant by traumatic injury, and a method used in special cases (instead of observing continuously on the same row) had been to take observations on successive rows on successive days, so that each row, instead of being handled once every day, was handled once in the month only. The cycle was repeated in the next month, and on those lines, although sampling error was increased to some extent, the damage factor was almost completely avoided.

MR. YATES, in answer to a question by the Chairman, said that there did not appear to be any statistical offence in observing one row for a month and then moving to another, though the interpretation of the observations would be more difficult than when observing one row constantly or changing at every set of observations. For most purposes, however, he preferred the method of making a new random selection of plants at each sampling, since that both minimised the chance of damage due to previous observations and gave a more representative set of values over the crop as a whole. The particular method adopted, however, depended very much on the sort of information that was desired, and the method that was best for one measurement might not be so for another. In the sampling observations on wheat of the Agricultural Meteorological Committee's phenological scheme (which he went on to describe) a new set of samples was observed at every date: this might be expected, for instance, to give a better determination of maximum shoot number, but a less accurate determination of the date when growth (in height) was at its maximum.

Mr. Yates also drew attention to a useful method of determining the best sampling process for any particular crop, namely that of harvesting the whole of one or more plots, in small units, so that different sizes and types of sampling unit could be tried in the statistical laboratory. This had been done by Kalamkar\* for the wheat plant and had resulted in a very considerable simplification of the sampling process.

Mr. Yates then went on to discuss Mr. Heath's paper. He congratulated the author on the excellence of his work and in particular on having taken the trouble to take two or more samples selected at random from all possible samples from each plot so that the proper

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\* R. J. Kalamkar. "A study in Sampling Technique with Wheat," Journ. Agric. Sci., Vol. xxii, 1932, pp. 783-796.

sampling error could be determined. Such a random selection was always worth while because the accuracy of the sampling process could then be reviewed (possibly by some future worker) and little extra field work was involved.

There was one small technical point that should be mentioned. The estimates of the efficiency of the sampling process given in column 10 of the table of Mr. Heath's paper were underestimates for two reasons (1) because the number of the samples was limited, (2) because the errors of estimation of the experimental error introduced a bias into the percentage efficiencies. On correcting for these factors the mean efficiency (as compared with measurements over the whole plot) of the dry weight sampling (over all experiments) was found to be 49.9 per cent., and that of the height sampling 57.5 per cent., as opposed to the mean efficiencies from Mr. Heath's values of 36.8 per cent. and 45.7 per cent. respectively.

DR. SANDERS stated, in reference to what Dr. Balls had said, that their object in the experiments he had described was to take a random sample at each count, because in that way a good representation of the plant over the plot at the time was secured. With the introduction of the co-variance method it was found that if the same samples were not taken each time, there was a very material reduction in the correlation coefficient, and consequently the co-variance system lost a lot of efficiency. That was their experience in wheat experiments, and he thought the same applied to cotton.

## SECOND DAY.

### COTTON PESTS.

*Chairman :* DR. S. A. NEAVE.

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#### FIELD STUDIES ON THE RELATION OF INSECT PESTS TO CLIMATIC CONDITIONS, WITH SPECIAL REFERENCE TO COTTON.

BY

C. B. WILLIAMS (*Rothamsted Experimental Station*).

In the early days of medicine the business of the doctor was to cure his patients when they became ill. Later, with an increase of knowledge and facilities for research, a large portion of his time was spent in finding the causes of disease and why his patients became ill. Nowadays medical science is largely concerned with the prevention of disease.

In economic entomology one sees the same trend of affairs, although considerably behind medicine in achievement. As an insect only becomes a pest when it increases largely in numbers, many investigators are now considering from various aspects the problem of changes in numbers of insects.

Some entomologists think that most often the numbers of any one species of insect vary owing to alterations in the abundance of its parasites or other enemies. Others think that the main causes of variation are to be found in changes of climate or weather. The truth lies probably between these two extremes, but still variations of climate remain as factors of major importance, as they can act not only directly on the pest, but also indirectly on it through their action on its parasites and predators and on its food supply.

Of recent years much detailed and accurate work has been done in laboratories on the influence of single factors—temperature, moisture, or light—on some particular insect, and the results have been very striking. But work in the field has seldom, if ever, reached the accuracy of laboratory work, nor has it been carried on for a sufficiently long period to cover the more likely permutations and combinations of the factors involved. In fact one usually meets the criticism that field conditions are too complicated to be analysed. Still, if the results from the laboratory are ever to be applied in the field, a start must be made to collect field information with the greatest possible accuracy, even if the attempted analysis only shows the flaws in the methods and so leads on to better ones.

In order that it may be possible later on to get the greatest amount of information from the observations, these should include :—(1) The insect, and its variation in numbers, activity, and its stages of development ; (2) The development of the crop concerned, and also of the other major hosts of the insect ; (3) As many as possible of the climatic factors.

If the interest of the investigation centres round one crop—as in the present case, cotton—a general survey should also be made of the climatic relations of the crop in different parts of the world, and of the presence or absence of each pest under the varied conditions.

To consider these four questions in more detail : observations of the insect should always be made at regular, and, if possible, frequent, intervals, and should be of a form that can be expressed as numbers or at least as dates. Often the same set of figures can be interpreted in both ways. Thus, in Egypt we took observations at weekly intervals on the percentage of green bolls in different parts of the country damaged by pink boll worm, from 1918 to 1924 (*see* Fig. 1). These can be dealt

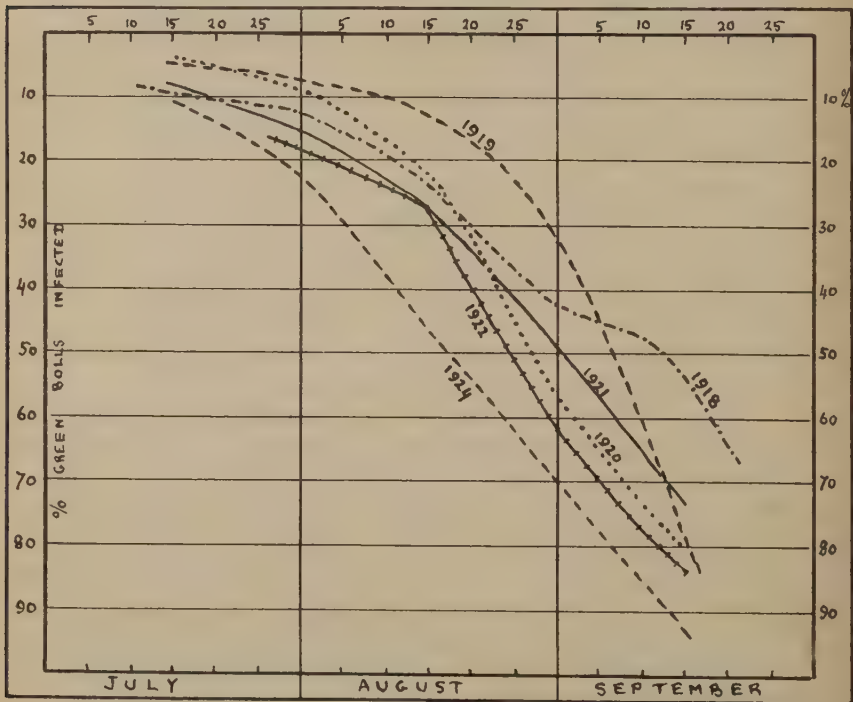


FIG. 1.—Percentage of green bolls of cotton in Lower Egypt attacked by Pink Boll Worm in certain years between 1916 and 1924.

with in two ways. Either we can compare the different percentage infection on the same date in different years ; *e.g.*, on the 1st September the infection was 33 per cent. in 1919, 50 per cent. in 1921, and 70 per cent. in 1924. Or, on the other hand, we can compare the dates in different years on which a particular degree of infection was reached ; *e.g.*, 50 per cent. infection was reached on 16th August in 1924, on 1st September in 1921, and on 12th September in 1918. The latter method gives a measure of the earliness or lateness of the event, which is of value for phenological work.

It is possible in the recording of observations to make use of standardised traps, and we are at present studying the use of light traps at Rothamsted. The difficulties in this connection are two-fold : first, that the catch depends partly on population numbers and partly on activity ; and secondly, that a very efficient trap may very markedly reduce the population in its vicinity.

It should be unnecessary to mention in these days that any sampling method in use should be reduplicated, at least, for a period, in order that the error of measurement may be estimated, and indeed that the whole technique should be studied by a competent statistician to see that it will give reliable information along the intended lines.

With regard to the plant, the following of the different stages of growth is more a matter for the botanist than for the entomologist, but if co-operation with a botanist is impossible, the investigator must himself see that a record is kept of such points as dates of planting, flowering, and fruiting, together with numbers of flowers and fruits on specified days for comparison with other seasons, and, of course, records of yields of the plots concerned.

It is often very difficult to get definite points in the growth of a plant for registration. In the observations quoted above on the pink boll worm in Egypt, when we came to analyse the results we found it almost impossible to find any regular observations on the state of the maturity of the cotton plants over the same period—observations that we had ourselves neglected to take. After many trials we were finally driven to use the date of the commencement of ginning at one of the government ginneries. This factory was less influenced by commercial considerations than most, and, further, it always started work when a fixed large amount of picked cotton was in store. Thus if picking had started a little too soon for the season, it would take a few days longer to amass the required reserve and so the error would be to some extent compensated.



From these somewhat unsatisfactory figures we finally obtained the data shown in Fig. 2 which demonstrate how closely the earliness

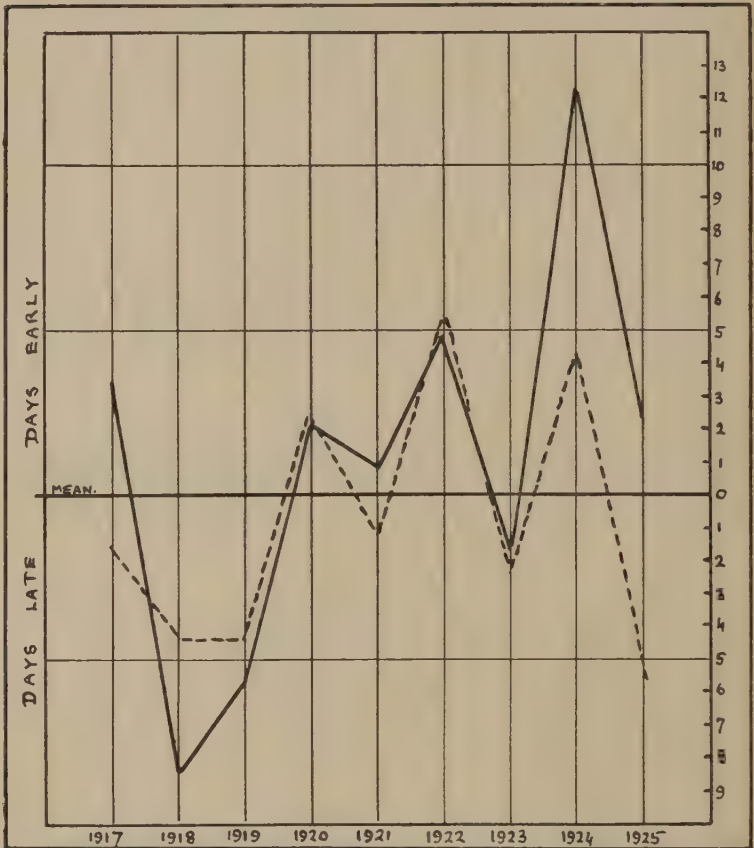


FIG. 2.—Departure from mean date of the development of Pink Boll Worm attack in Lower Egypt (solid line) and of the date of starting ginning at Sakha (dotted line), 1917 to 1925.

and lateness of the pink boll worm attack in each year was associated with the earliness and lateness of the cotton crop, almost certainly through the similar action of climatic conditions on each.

Turning now to the observations of climatic factors, many of these are already regularly taken at meteorological stations under, of course, standardised conditions. The extent to which they can be taken specially for an entomological investigation depends largely on finance, and particularly on capital outlay, because as many as possible of the factors should be measured by recording instruments.

With regard to temperature observations, these should include air-shade temperatures, and soil temperatures at more than one depth, all by recording instruments, while black bulb maximum and grass minimum temperatures should be read once a day.

Humidity can be recorded continuously by a hair hygrometer (moderately reliable except with extremely dry conditions) or can be read at intervals with a whirling hygrometer of the egg-whisk type, or by a dew point apparatus. Stationary wet and dry bulb thermometers do not always give reliable readings. Humidity can be expressed as relative or absolute, or as saturation deficiency, each of which may give a different form of correlation with insect numbers.

Radiation is often of importance, but its measurement is difficult and unsatisfactory. Radiant heat is measured by the non-maximum black bulb thermometer at any one moment, or integrated by such an instrument as Wilson's radiation integrator, improved considerably by Buxton but still far from satisfactory. Light can be measured by several photometers on the market, many expensive and bulky, and few reliable under field conditions. The number of hours sunshine can be measured by the Campbell Stokes Recorder, which is reliable but not cheap; while moonlight, believed to be of importance in determining the night activity of certain insects, can be recorded by an instrument recently designed by us for the purpose at Rothamsted. Loss by radiation at night is practically unstudied, but can be estimated by the difference between air-shade and grass minimum temperatures, and is also intimately connected with the amount of night cloud.

Other factors which must be measured, and recorded if possible, are rain, wind (force and direction), irrigation (if any), cloud (by day from the sunshine recorder or by a nephoscope, and by night by photographing certain star tracks), and such factors as evaporation, which are really re-combinations of other factors in various proportions. For measuring evaporation in the cotton crop in Egypt I found the "Piche" evaporimeter a cheap and useful instrument.

This list of factors will seem a very formidable one, and to a resident in temperate climates probably unnecessarily large. In the cooler parts of the world temperature is of such overwhelming importance in determining natural events that one is apt to overlook the fact that there are enormous areas where it only plays a minor part. I can imagine no more interesting and fertile subject for study at a tropical research station than the causes of seasons in the tropics. It would most assuredly be a "long range" piece of work, but of fundamental importance to agricultural science.

It has often been emphasised that conditions within the crop do not resemble closely those standardised by meteorologists, but while this is true, there is probably some relation between the two, and it is the business of the agricultural meteorologist to study this relation closely so that his observations can be applied to, and can make use of, the vast accumulation of standardised data.

Turning now to the climatic relations of a single crop, I published in 1923 (*Egyptian Ministry of Agriculture, Tech. Bull. No. 32*) a short account of the relation of the cotton plant to temperature and rainfall in different parts of the world. The temperature diagram is reproduced in a simplified form in Fig. 3. This indicates that cotton is planted in different countries over a wide range of mean temperature, in fact from 50° to 90° F., but that conditions during the late maturing period and the commencement of picking are remarkably uniform (with the exception of Iraq). One would expect from this that the pests of seedling cotton would vary considerably from country to country, but that the pests of the older bolls would be widespread and more uniform in type, except in so far as that they must survive or avoid the adverse conditions during the no-cotton season. I have never had an opportunity to carry on this line of investigation, and so far as I am aware it has not been followed up by others.

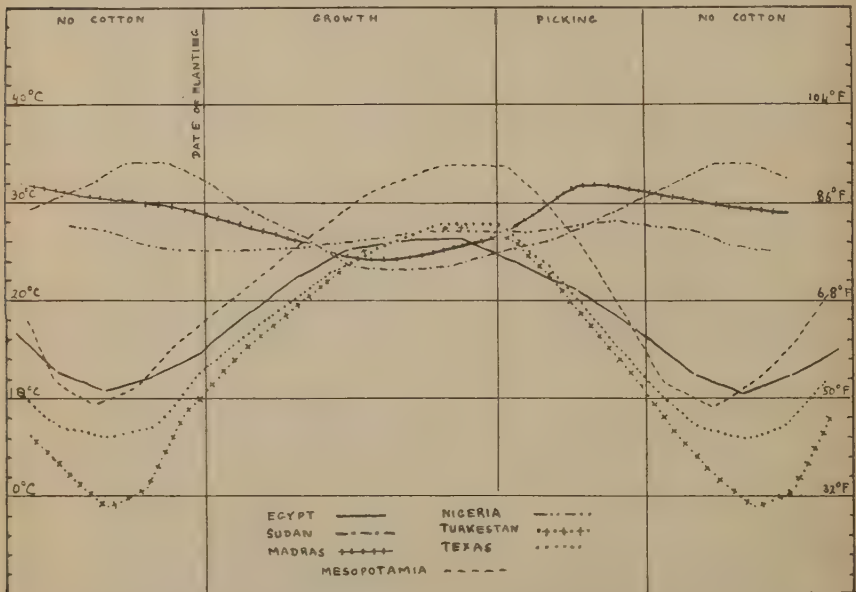


FIG. 3.—Mean air-shade temperature changes in various cotton growing districts arranged to correspond with the development of the cotton crop.

Field observations on the relation of insect pests to environment are difficult to plan and often tedious to carry out efficiently and regularly over a long period, but the interpretation of the results is of the greatest interest, and the longer they are continued without a break the greater the interest becomes. In fact (as in the case of the classical experiments at Rothamsted) long-continued observations often throw light on problems unsuspected at the commencement.

Perhaps I might conclude by answering in advance the critic who asks of what practical use is it to know the influence of climate and weather conditions on insect pests, as we cannot alter the sun, the wind and the rain. Such a man does not realise what a large part of the agricultural man's life is spent in altering these very factors, if not at their fountain-head at any rate as they reach his crops. By drainage he alters the available rain, by distance of planting, shade plants or wind breaks he alters the available light and humidity, by choice of time of planting he arranges temperature to suit the demands of the plant, by manuring he not only supplies food to the crop, but may alter the texture and colour of the soil, and hence its temperature at various depths. And even if we cannot do all we would like to do to-day, research on the influence of climatic conditions is bound to progress slowly, and by the time we have the information ready for use, research along other lines may have given the planter still other weapons to bend the climate to his will.

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DR. C. B. WILLIAMS, in introducing his paper, said there had been a great dispute for many years as to the part which climatic conditions played in the variation of numbers of insects, and the general opinion was veering round to the position that climatic conditions were one of the main determining factors; because, even when parasites were one of the factors which caused the alteration in abundance of insects, the climate acted on those parasites as well as on the insects themselves.

In this country we were apt to over-emphasise the importance of temperature as the one main climatic factor, but in the tropics and sub-tropics temperature became of less importance, and other factors, such as rainfall, humidity, and intensity of sunlight, of major importance.

In working out a problem it was necessary to get some measure of insect abundance, to know the condition of the crop concerned, and as full a measure of the climatic factors as it was possible to get.

The paper contained one or two examples from the work which he did in Egypt, showing how continuous records taken under standardised conditions would eventually, if carried on long enough, produce interesting results. The most striking was the case of the

earliness or lateness of the attack of pink bollworm in Egypt, which was worked out statistically. Over a period of about eight or nine years there was seen to be a very close relationship indeed between the earliness and lateness of the attack of pink bollworm and the earliness and lateness of the cotton crop itself, the explanation being either (1) that the earliness and lateness of the cotton crop determined that of the pink bollworm, which was possible, or (2) that the pink bollworm determined the cotton crop, which was not likely, or (3) that they were both dependent on a third factor, namely, climate. There was evidence to show that the first factor—the earliness and lateness of the cotton crop directly affecting the pink bollworm—was of quite serious importance, because the pink bollworm appeared in the fields before the cotton crop was ready for eggs to be laid on it, and simply hung on and then started a sort of geometrical progression of attack as soon as the cotton had reached the right stage.

The last diagram in the paper showed the result of his preliminary study of the relationship of the cotton to climate, which was done primarily with the intention of studying insect pests on cotton. The facts which emerged from it were obvious. First of all, cotton could be grown under three types of temperature conditions: (1) planted on a falling temperature and picked on a rising temperature, which occurred in parts of the Sudan; (2) planted on a rising temperature and picked on a falling temperature, which occurred in a good many countries, such as Egypt, Mesopotamia and America, and (3) it could be grown in countries such as those in tropical areas, where the temperature changes were comparatively insignificant.

One fact that came out was that in all those countries the temperature conditions round about the ripening of the bolls and the early picking were extraordinarily uniform, and one would expect, therefore, that insect pests would be more widespread when they attacked the bolls, but much more restricted in their habits when they attacked the seedling stage. Cotton could be in the seedling stage at temperatures ranging over something like  $15^{\circ}\text{C}$ ., whereas in the later stages the range of temperature was much smaller.

Dr. Williams said a very similar problem, but naturally not with cotton, was being studied at Rothamsted. They were trying to introduce into the field study of the relationship of insects to climate the same order of accuracy as was obtainable in the laboratory. To get an abundance of insects, a light trap was used. This light trap had been burning for nearly eighteen months. The trap also sorted the captures during the night into eight bottles, each bottle being roughly one-eighth of the night, so that in the morning it could be seen what insects had been caught and at what time of night they had come in.



Simultaneously, all the climatic factors were recorded, including the temperature, rainfall, wind and so on, but in addition the intensity of the moonlight and the amount of cloud at night.

At the end of the period they had a long series of observations on the abundance of insects, their activity and the climatic conditions. Departures for each of these from a 15-day mean were noted, and correlated mathematically. Some very curious results had been obtained. He only had time, however, to mention one of them. The departures from the 15-day mean of the numbers of insects for a period of a month were correlated with the departures of minimum temperature from the mean, and a high positive correlation, about 0.7, was found. Then the same departures of insects were correlated with the maximum temperature of the previous day and a low positive, but just significant (about 0.45) correlation was obtained. It was known that the minimum temperature and the maximum temperature themselves were correlated, so that was worked out and found to be positive and about 0.65.

There were three factors correlated—the abundance of insects, the minimum temperature and the maximum temperature; from those three by means of a partial correlation mathematically the influence of any one of the three on the other two was eliminated, and it was then found that the correlation with the minimum temperature was increased by getting rid of the maximum temperature effect; but when the minimum temperature effect was eliminated, the correlation was changed from a positive correlation of 0.4 to a negative correlation of about 0.35.

What that meant was, that on days of the same maximum temperature, there was a very high correlation of insect activity with minimum temperature; on days of the same minimum temperature there was a negative correlation with the previous day's maximum temperature. Taking all the days, the hotter the day the larger the catch, but taking days with only the same minimum temperature in the dawn and during the night, then the hotter the previous day the lower the catch; and that fact was completely obscured by the inter-correlation of these climatic factors.

Dr. Williams said he wished to emphasise the general importance of that observation, because it illustrated the absolute necessity of sorting out the inter-correlations of the climatic factors before any one of them could be correlated with insect abundance. That example could be repeated probably a hundred times in the cotton fields, and he hoped that something would be done on those lines.

## DISCUSSION.

MR. THOMAS said that the principal insect pest in the Punjab and in Sind was White Fly: the damage done by this insect in any year was, in his opinion, subject to two limiting factors—the climate and its natural parasites and predators. As regards the climate, evidence had been obtained that rain increased the mortality of the nymphs. The natural enemies of the White Fly also caused a high mortality in certain years, and the variations in these two factors, he suggested, largely determined the degree to which White Fly affected the yield.

Cotton in the Punjab also suffered from jassid, but its reaction, both to this pest and to White Fly, seemed to him to be different in the Punjab from what it was in other countries, such as Africa. A good deal of work needed to be done to clear up this point.

Having regard to the very serious loss to the grower that was occasioned by pests and by virus and fungoid diseases that were frequently carried by insects, he asked what steps the plant breeders were taking to test the resistance of new strains to the principal prevailing pests and diseases before distributing them for general cultivation. He would like to know whether it was feasible to test them, under controlled conditions, in respect of their power of resistance, more especially to the more insidious diseases that could be conveyed to the plant by insects.

MR. PARSONS said that it was common practice with all plant breeders to expose selected strains to the pest conditions existing in the locality where they were intended to be used. In the work of an Experiment Station the insects had to be studied continuously and intensively in association with the plant breeder. The field was to be regarded as a laboratory just as much as a room within four walls, and at Barberton they considered that all the conditions surrounding the crop had to be studied in connection with any plant breeding work, and their methods were adapted to that end.

MR. THOMAS expanded what he had recently said by an illustration. It was possible in the Punjab and Sind to have several successive years with very little jassid attack or White Fly. A plant breeder might select a superior strain in the first of several such years, and consequently by the time that the new strain was ready to be put out to cultivation on a large scale, it had never been exposed to serious pest conditions. Yet, if, in the first year in which it was distributed, White Fly or jassid incidence were heavy, the new strain would possibly not prove resistant, and the grower would suffer heavy loss.

MR. TROUGHT said that when he was in the Punjab, from the time of his arrival in 1915 there was no serious jassid attack until 1930. About the middle of that period he produced what he considered to be a promising strain, but realising that heavy jassid attack did occur from time to time, he had already bred from that strain at least one jassid-resistant selection before the heavy jassid attack did occur in

1930. *The problem of possible insect attack was constantly before the plant breeder. He gave as another example the selection in the Sudan of a strain which seemed to be distinctly promising for the Nuba Mountains district; it had been tested for the conditions that existed there for four or five years. Before distributing it on a large scale, however, he decided that it should be resistant to jassid. He therefore sent the seed to Nyasaland, where jassid attack was usually heavy. It was found to be jassid-susceptible, and the strain was dropped.*

MR. STEWART said Mr. Thomas had raised the question of White Fly and its possible connection with the periodic partial failures of American cotton in the Punjab. A.F., a very valuable variety of medium staple cotton, was put out by the Agricultural Department about 1914, and during the time which had elapsed since then, there had been, probably, six or seven years in which the results obtained had been very bad. The crop would appear quite good, with every prospect of a normal and in some cases a bumper yield, when suddenly about the end of September certain conditions arose, and within a week or two the prospect of a normal crop fell to one of a 25 per cent. or 30 per cent. crop. In due course the leaves turned brown and fell off: the bolls failed to open properly, and when they did open, the seed inside was undeveloped, and the lint itself was trashy and had no strength.

Some years ago, Mr. Thomas put forward the theory that in the Punjab those unfavourable conditions might be the result of an earlier attack by White Fly. The matter was still under investigation, with the help of a grant from the Indian Central Cotton Committee. Sprays had been evolved which gave almost a 100 per cent. kill of the insects and nymphs. When he left India in the spring, the result of last year's large scale experiments, where about 3,000 acres of cotton had been sprayed, was not available. Figures showed that the kill which they had obtained by means of sprays was almost complete, but the final figures of yields were not available, so that the effect of killing this pest on the out-turn of the crop was not known. The Indian Central Cotton Committee had provided funds to enable the experiment to be repeated in the coming year, and it was hoped that a definite conclusion would be reached as to whether the White Fly was or was not responsible for the partial failures of cotton.

These failures were being attacked from various other angles as well, and recently a big scheme had been sanctioned and financed by the Indian Central Cotton Committee to investigate the matter on physiological lines.

In connection with the subject matter of the paper under discussion, Mr. Stewart added that in the Punjab, at any rate with the pink bollworm, there was a very close connection between climatic conditions and the incidence of the pest. This matter had been under investigation for some years, and it had been proved conclusively

that in the localities where the pest was very severe one set of meteorological conditions—temperatures, humidities, and so on—existed, whereas in those neighbourhoods where it was less severe a totally different set of conditions existed.

In the course of last year's investigations, one very important point seemed to emerge, but it still required to be proved finally. The entomologists found that in those localities where the incidence of the pest was low, and where the meteorological conditions were recorded as unfavourable for its development, a relatively very high proportion of the males of the pink bollworm were found to be infertile.

- DR. WILLIAMS said that one of the most remarkable instances of the influence of climatic conditions on the pink bollworm was the geographical distribution of the resting stage. The pink bollworm could go into the resting stage between the double seeds, but whether it did so or not apparently depended largely on the climatic conditions, and the presence or absence of that resting stage completely altered the whole problem of practical control. In Egypt they had the resting stage, and they had it in the Sudan also, but in the latter country it was at the hot time of the year. In the West Indies it was non-existent and in Coimbatore it was practically non-existent, but it occurred, he believed, in the Punjab. Apparently in drier areas the insect went into the resting stage; and in that resting stage it could live from six to eighteen months, and be carried from one country to another: it was clearly, therefore, an extremely important factor.

With regard to the influence of climatic conditions on the pink bollworm, MR. EVELYN said that in St. Vincent, prior to 1929, cotton was always planted in the month of June, and losses from pink bollworm were extremely heavy. The planting date was then changed to September, and since then the loss from this pest had been almost negligible, sufficiently so to justify them in doing no further work on it in the island. It now arrived in the crop at too late a stage to do much harm.

- DR. KEEN said he wished to congratulate Dr. Williams on his paper. Even at the present time, it was not often that the practical man, the grower and the scientist found in the same piece of work something that suited them all.

He was much impressed by the courage with which Dr. Williams had undertaken a wide series of meteorological observations. He wished to support his view that as many of the factors as possible should be measured by recording instruments. With a recording instrument it was possible to turn back to the graphs years later and extract from them something which possibly had not been thought of when the investigation was started.

He also wished to commend for general work some form of evaporimeter. Dr. Williams had found the Piche evaporimeter cheap and useful. For some years at Rothamsted a simple form of the



## RELATION OF INSECT PESTS TO CLIMATE.

*Livingston* evaporimeter had been used, which had given important data with regard to soil moisture and evaporation studies. The simple form used was merely an inverted porous candle filled inside with water and supported in a cork over a bottle of water. The bottle and candle together, being in one unit, were weighed daily, and the daily loss of weight gave an integrated measure of the evaporation over the day. It was a very simple instrument and required very little attention.

The main difficulty about such work, of course, lay in the attempt to correlate meteorological information, which was by its very nature numerical, with the more diffuse and qualitative information which one got on the phenological side, whether one was studying the growth of the plant or the behaviour of insects. Dr. Williams' work with an illuminated trap was very important in this connection, because by ascertaining the numbers of insects and their time-distribution over the night, he was getting his data in the two forms in which the entomological aspect of it could be used. Both of those factors could be expressed as numerical values, and it was possible, therefore, to explore correlations between them and the numerical values of the meteorological conditions.

As Dr. Williams had pointed out, correlations must be used with caution. A correlation between two factors did not necessarily mean that one controlled the other. Each of them might, in reality, be controlled by another factor that had not even been measured in the experiments.

MR. SUNMAN said that in Kenya it was important to breed wheat for resistance against physiological forms of rust. For this purpose wide use was made of laboratory seed inoculation and epidemics produced artificially in the field. He asked whether any similar technique was employed in breeding cotton strains for resistance to virus and other diseases, or whether in testing such resistance the plant breeders relied entirely on such epidemics as occurred naturally from time to time.

MR. BAILEY referred to the question which had been asked as to how far it was possible, in the ordinary technique of plant breeding, to introduce artificially, the conditions of pest incidence which new varieties of cotton would be required to withstand under field conditions. This was certainly possible up to a point, and was actually being carried out at the moment at the Plant Breeding Station at Shambat. The particular point of interest there was the production of strains resistant to "Leaf curl" which is carried by the White Fly. Steps are taken, by the growing of diseased ratoons and by the planting of cover crops favoured by White Fly, to keep up a pool of infection from year to year. It was interesting to note that, though the procedure undoubtedly raised the general level of infection, it was impossible to guarantee what might be called a "maximum" infection in every season, and in certain years the level of infection was comparatively low. The



system was, of course, not without its dangers, and steps must be taken to see that the diseases, deliberately encouraged in this way, did not spread to the main cotton areas.

SIR GEOFFREY EVANS said it was evident from Dr. Williams' paper that the old methods of entomological control were disappearing, and that investigators were taking a much wider and broader view of their problems. That was perhaps only natural, because entomology had developed very rapidly in the last few years, and it showed signs of producing sub-sections such as, for example, the section that dealt with biological control, and the biological control men themselves were agreed that, when any pest was being investigated, the ecological conditions which controlled the incidence of the pest were the first matters that had to be ascertained. They had repeatedly come to the conclusion that it was useless attempting to introduce a parasite until they knew exactly how the pest lived and under what conditions it thrived. There were instances of parasites having been sent out which had failed, and he thought they had failed largely because the right period in which to introduce them was not known, nor the conditions under which they were most likely to succeed.

A proposal had been put forward to try to control, by means of parasites, the pink bollworm, which caused damage in the northern islands, and particularly in Montserrat, but it had been decided to make a thorough investigation of the pest in the first place, as, apart from St. Vincent, where the matter seemed to have been settled, at any rate for the time being, by the change in the planting date, in the northern islands the pest appeared, apparently, periodically. For three or four years no damage was sustained, and then there was a very bad attack. It was necessary to discover what the conditions were that were causing these sudden attacks.

He was very glad to see the growing tendency towards team work in tackling entomological problems. The Empire Cotton Growing Corporation were setting a very good example in that way. At Barberton the entomologists were closely associated with the plant breeding staff, and the Corporation had even given a man who had been trained as a plant breeder an intensive course in entomology, with a view to dealing with one of the important cotton pests that occurred in Africa.

DR. BALLS mentioned a striking fact illustrating the point raised by previous speakers as to the incidence of White Fly, Leaf Curl and Blackarm. The Sudan was very severely troubled by all three. Some 2,000 miles down the Nile, in Egypt, they had Blackarm, White Fly and Leaf Curl, and had had them all for thirty years. But there Blackarm attacked the cotton out of season, when it did not matter; the White Fly only carried Leaf Curl to hollyhocks—the virus never infected the cotton. That was an amazing example, on a gigantic scale, of the effect of climate on disease.

MR. MASSEY said that in the Sudan, both Leaf Curl and Blackarm were very seasonal. In the early work on Leaf Curl no infection could be obtained from White Flies occurring at the Experiment Station at Shambat. As regards Blackarm, this disease was most severe in the Gezira, where cotton was sown during the rains. It was of little or no importance where rains were light or occurred late in the season.

**CLIMATIC AND SOIL FACTORS IN RELATION TO PREVALENCE OF, AND DAMAGE BY, INSECTS; POSSIBILITIES OF REDUCTION OF LOSS OF CROP DUE TO INSECTS, BY THE SELECTION AND RESTRICTION OF SOWING PERIODS AND BY THE PROPAGATION OF COTTON STRAINS BETTER ADAPTED TO LOCAL CONDITIONS.**

BY

H. HARGREAVES (*Uganda*).

In considering the first part of my subject, observations are confined to insects of the sucking type. There appears to be very strong evidence in support of my view that certain soil and climatic conditions are conducive to a radical alteration of the composition of the sap, which renders plants more attractive to insects, and which stimulates rapid reproduction of such sap-feeding insects.

Taking the case *Lygus vosseleri*, a Capsid bug which feeds on the young buds, bolls and leaves, and which during certain seasons is capable of causing a loss of something like 25 per cent. of the cotton crop in Uganda, the prevalence of the bug and its damage are usually coincident with, or following on, periods of rainy weather during which rapid (not necessarily healthy) growth of the aerial parts of the plant occurs. Observations indicate that there is a correlation with soil as well as with climate; the incidence of *Lygus* on cotton grown on land which has been under continuous cultivation for several years is usually much higher than on more recently opened up land.

With *Helopeltis*, observations indicate that water-logged soil, shallow distribution of roots due to shallow soil or to a hard layer which the roots have been unable to penetrate, are factors which predispose plants to attack. The susceptibility appears to be connected with a reduction of the water supply (and the substances dissolved in it) to the aerial parts, caused either by the inability of the roots to function properly or to actual scarcity of water in the soil. Penetration of roots into the soil, provided the latter is of adequate depth, is dependent on such factors as the nature of the soil, the depth of cultivation prior to sowing, and the amount and distribution of rain before and after sowing of seed.

Prevalence of jassid bugs on cotton seems usually to be correlated with poverty of soil and with the inadequacy of water-supply during dry weather. The direct effects on plant growth of poor soil, bad soil conditions, and of continued dry weather are liable to be attributed (erroneously in my opinion) mainly or solely to attack by jassids.

During dry conditions the incidence of aphids increases enormously on young plants, and these insects must seriously retard growth. With the advent of rain a marked reduction of aphids occurs almost immediately, and, if wet conditions continue, a fungal parasite may cause death of the remaining aphids. The factors involved in this instance are not very clear, but the small development of the root system in young plants may primarily be responsible.

Although white fly (*Aleurodid* bug) is of little or no importance on cotton in Uganda, it furnishes a further example of correlation with climate, since its numbers increase markedly during periods of dry weather.

The prevalence of a leaf-feeding spider-mite (species undetermined) which causes yellow mottling and, finally, death of the leaf, appears definitely to be correlated with poor soil and dry weather conditions.

The stainers (*Dysdercus spp.*) cause much greater damage to cotton bolls during cooler and wet periods than during hot dry weather, though this coincidence is not necessarily correlated with increased prevalence of such stainers. There is, of course, the complication that the damage by such insects is indirect through introduction of boll-rots, and this may account for the fact that the amount of damage is, to some extent, independent of the number of stainers.

Passing to consideration of the second part of the subject:—possibilities of reduction of loss of crop due to insects, by the selection and restriction of sowing periods and by the propagation of cotton strains better adapted to local conditions.

With a crop such as cotton it is essential to have dry weather for the opening and final development of the bolls. The longer the series of sowings in adjacent plots, the greater the possibility of increased damage to the later sowings by insects bred up on the earlier sowings. The effect of lengthening the sowing period is to some degree comparable with that of growing strains of cotton which take unusually long periods to mature; in both cases a continuous supply of food is available for the pests, but the effect of a prolonged sowing period is even more adverse to the crop than is the use of slow-maturing strains, since in the former case an insect which feeds only on (for example) young bolls is provided with plants which in succession are in the required stage of growth.

While agreeing that the native must first ensure the supply of his own food and that weather also is a factor which limits time of sowing, it appears to me that during average years the sowing period might, without detriment to the area put under cultivation or to the general growth of the crop, be compressed in Uganda from five to four months. I admit that it is difficult, if not impossible, to forecast rainfall distribution, and that during occasional seasons of erratic weather an increase in yield has apparently resulted from an extended sowing period, but it is questionable, especially where pink bollworm is present and a long close-season is very desirable, whether, over a period of years the longer sowing period is really beneficial in quantity and quality of crop.

In Uganda it is usual to have copious general rain during May, and cotton sown during such conditions is likely to suffer especially owing to attack by *Lygus* causing death of the buds which would otherwise produce the lower fruiting branches: in effect, the first crop of flower buds is prevented from developing, and the growth produced during later periods, when *Lygus* is scarce, must be relied on to produce the crop. Where, as in Uganda, there are definite and more or less constant differences in the incidence of rainfall in various areas, it would be unwise to define a single sowing period for the whole country; sowing periods must be defined separately for each climatic area.

During a normal season when the rainfall is moderately heavy, early sowing tends to result in a longer ripening period than does later sowing, thus exposing the plant to attack by insects over a longer period and permitting stainers (and probably pink bollworm also) to increase in numbers. Moreover, a great proportion of early-sown cotton may mature before the weather becomes very hot and dry; this allows of further increase in damage by stainers and pink bollworm.

It has been shown above that in Uganda (in the absence of earlier sowings) a later sown cotton is likely to suffer less damage by insects and that a long growing period is undesirable because it increases the period of exposure to pests and favours multiplication of the insects. The propagation of early-maturing strains which are satisfactory in regard to quality and quantity of the lint produced should result in a marked reduction of damage by insects and it may allow of later sowing and thus tend to extend the close season for cotton.

I have remarked above on the tendency to attribute the effects of unfavourable climatic and soil conditions to attack by jassid bugs. But, while bearing this in mind, there is no question that certain strains of cotton ("jassid-resistant" types) suffer less from the effects of such



conditions and of jassid bugs. I consider, however, that the "resistance" is a result of better adaptability of such strains to the particular environment, and that they are drought-resistant strains. Adopting this view, it follows that the more suited a strain is to the environment of a given area, the more healthy will the plants be and the less likely to suffer damage from certain sucking insects (and diseases).

If it is possible by alteration of soil conditions to counteract, in part at least, the effects of unfavourable distribution of, or inadequate, rainfall (for instance, by increase in humus or by cultural conservation of soil moisture), it is feasible that "jassid-susceptible" cottons—possibly of greater commercial value—might be grown successfully in somewhat drier areas.

The paper was introduced by Dr. Tothill, who said that it contained a number of contentious points, and he hoped that it would produce a useful discussion.

#### DISCUSSION.

MR. PARNELL quoted a paragraph from Mr. Hargreave's paper in which he said that "Prevalence of jassid bugs on cotton seems usually to be correlated with poverty of soil and with the inadequacy of water supply during dry weather," and to a further paragraph in which he said that in his opinion jassid resistance was the result of better adaptability of such strains to the particular environment, and that they were drought-resistant strains.

While agreeing to some extent with these views, Mr. Parnell did not think that they went far enough to explain jassid resistance entirely. At Barberton they found that the worst jassid attacks occurred after heavy rains. The connection with drought might also be seen there, but in his view the connection was not definitely with drought as drought, but with an unhealthy condition in the plant, as Mr. Hargreaves suggested. He mentioned that he had also seen a connection between jassid attack and poor soil and also water-logged soil. He further agreed with Mr. Hargreaves that susceptibility to jassid might be associated with unhealthy conditions of growth, but he did not believe that resistance was actually due to better adaptation to environment, so far as the material handled at Barberton was concerned and in their conditions. They had tried strains from many different parts of the world, and some of the American types grew extremely well, so that there could not be any question of their being unsuited to their environmental conditions; nor did they suffer severely from drought, yet they invariably succumbed to jassid. Their only defect, so far as he could discover, was their lack of any really thick covering of hairs on the leaves. At present they had not gone very far in determining exactly what it was that made the plant become



susceptible to jassid in the course of a season. Susceptible types generally began to suffer when they were more or less full of crop and starting to ripen. Very susceptible types suffered before then, but a resistant type, which might suffer to some extent, did not begin to do so until the crop began to ripen. A plant showing vigorous vegetative growth did not appear to attract jassid, nor did the insect appear to breed on it. They had not had time to investigate this point further as yet. One of the lines that he would like to follow up was the variance in the pH value of the cell sap. He would be glad to know of any work that had been done on this point.

MR. THOMAS said that in the Punjab and Sind they found that although the crop might do well up to the time of the rains, jassid attack began to be seen very shortly after their arrival. Last year the rains arrived in July, and they had a dry spell until early in September. The jassid effect was then still visible, but it was greatly intensified by the showers which occurred in September. He had noticed each year that towards the end of September or the beginning of October, the effects seemed to disappear, although the jassids were still present: he would much like to know if anyone could suggest the reason for this.

Mr. Thomas also asked Mr. Parnell whether he thought that there was anything more than a correlation between hairiness of leaf and jassid resistance, and whether hairiness in itself prevented the jassid from breeding on the leaves. Under field conditions he had seen great differences between plants with regard to their resistance to jassid, but had failed to find much difference between them on examining the relative hairiness of their leaves.

MR. PARNELL replied that he was inclined to the opinion that hairiness had a direct effect on the jassid, though he was not yet prepared to say definitely what that effect was. They had carried out experiments in which they had starved jassid nymphs for some time and then put them on hairy cotton leaves and much less hairy cotton leaves under small cages. Those put on the smooth leaves at once began to feed without any hesitation, but those put on to very hairy leaves at once began to show their objection. They did not seem to be at all happy, and continually kicked as though they were being tickled. They did not begin to feed for a very long time, and even then did not feed steadily. These experiments led him to believe that there was a direct connection between hairiness and jassid resistance.

Mr. Parnell said further that in an ordinary mixed crop there was a great variation from plant to plant as regards jassid resistance, and the connection between heavy crop and susceptibility was very definite. Other things being equal, a plant carrying a very small crop was much less susceptible than one carrying a heavy crop. This constituted one of their difficulties at the beginning of the work at Barberton, as it made it hard to find types which combined high yielding properties with good jassid resistance. Speaking generally,

they had found in making selections that hairiness and resistance were very closely connected—so much so that when there had not been a sufficiently heavy incidence of jassid, they were able with success to continue the selection work by looking only at the degree of hairiness of the leaves.

MR. JACKSON thought that the discussion on this and the previous paper illustrated the great importance of approaching the problem of insect pest control by means of different routes. Various speakers had stressed the influence of climate, others the influence of soil conditions. He believed that those two lines of approach offered the greater possibility of controlling pests than more direct methods of attack upon the insect itself. There was a wide field for further investigation on environmental conditions. A plant had its soil environment and its climatic environment, and the insect pest had its plant host environment and its climatic environment, and he felt sure that the most hopeful lines of attack would necessitate a study of all these conditions.

DR. BALLS said that at Giza, in the course of their work on the root system of cotton, which had gone on for many years, a number of observations had accumulated regarding the susceptibility of the plant to insect attacks when the root arrived at an unhealthy environment in the soil.

One field on the Giza farm was very interesting. Given certain times of sowing and certain weather conditions, the south side of a certain long, narrow area was soaked with aphids, while thirty yards away along the north side of the field, not a single aphid could be found on the seedlings, which, being healthy, were twice the size of those on the south.

This farm had been carefully mapped to depths of two metres below the surface, so that the position of the hard and soft layers was known. On the south side the roots at a certain stage arrived at a layer of clay, only with difficulty penetrable by the root systems. While they were worrying their way through, they were in an unhealthy condition, and, if other conditions were suitable for green fly attack, the green fly came *en masse*.

A further point which had been followed up at Giza was that, as the soil profile changes—and it usually did change as one passed downwards—so the reaction of the plant to its locality changed. If there were a layer of clay, say, half a metre thick, at a metre below the surface, then about a month after sowing, under Egyptian conditions, the roots of the seedlings would have reached that clay layer. The plants would then be growing under very different conditions from those which obtained when they were living in the soil in the top layer. While the plants were worrying their way through the clay layer, their growth was checked, and various things happened. For example, they might be attacked by diseases. The roots eventually emerged and came to healthy soil below; there they had got back

## INSECT PEST PROBLEMS IN UGANDA.

again into healthy soil conditions. It had been possible, with the help of air photographs, to trace the substructure of the soil from the reaction of the crop above ground.

Dr. Balls said he was inclined to think that in some of the cases to which reference had been made, where in certain years the crop had misbehaved towards the end of the season, especially when it had promised very well earlier, it was not impossible that the roots had gone to unusual depths and got into trouble from their excess of zeal.

He had perhaps put that in a slightly exaggerated form, but the conception of the plant as living effectively at different levels of the soil during the different stages of its development had proved of great value. The fact that a plant had started well was no guarantee that it would not go wrong, nor even that it might not recover again afterwards.

DR. WILLIAMS said that his way of looking at the problem was that the pest had its optimum environmental conditions and the plant had its optimum environmental conditions. If those two conditions differed, then the pest could be attacked by agricultural and ecological methods, because one could move from the optimum of the pest towards the optimum of the plant and thereby reduce the pest without sacrificing the plant. There were quite a number of insect pests, however, whose optimum conditions coincided with the optimum plant conditions, or had no relation whatsoever to them; in such cases the ecological methods of plant control were not of much value, and one had to rely on the older methods of direct slaughter. That was one reason why, before recommending any method of control, it was necessary to get an idea of the environmental conditions. It was only in that way that one could get a clue as to whether to advise watching and altering conditions, or whether to advise immediate expenditure on direct control.

The following paper and that from Northern Rhodesia, which succeeded it, were discussed together :—

## INVESTIGATIONS ON INSECT PESTS OF COTTON.

BY

F. S. PARSONS (*Barberton*).

At the commencement of work on cotton in South Africa by members of the staff of the Empire Cotton Growing Corporation it was recognised that if cotton was to be grown at all, means must be found to minimise the damage to the plant caused by a jassid bug. As is well known, Mr. F. R. Parnell, working at Barberton, has since bred out productive strains of cotton which are highly resistant to the jassid, and the

provision of these strains has made possible the growing of cotton in certain parts of the Union and neighbouring territories. There remained, however, other pests, principally cotton stainers and the American and red bollworms of cotton, which damage the crops from time to time to the extent that the maintenance and expansion of the industry are threatened. Investigations on these other pests have been in progress at Barberton for the past four years.

Recently, as a fundamental part of the work on cotton pests, we have inaugurated a system of continuous surveys of the course of pest activity throughout the year under variable conditions of climate and host. The surveys, which are largely of an ecological character, are now in continuous operation at points in the Transvaal, Natal and Swaziland, and the system is also being applied by our colleagues in Northern and Southern Rhodesia. We are acquiring from these surveys an extensive body of essential data, which have added value because of the close co-ordination of effort in the several territories. The data are serving to state the nature of the real problems, and point to promising lines along which to attack them. A description of this work and results is contained in the Corporation's publication, *Reports from Experiment Stations, 1932-33*, from which an extract is annexed. It is hoped that the methods and scope of the systems employed and the nature of the information accruing from them may be of interest to others attending the Conference, and that the occasion may be a favourable one at which to invite discussion of these and other methods of approach to insect problems.

The object of these remarks is to introduce a discussion by the author of the lines of investigation followed, and the inter relationships of the insect-plant ecology involved. These may be stated as follows with a view to focussing attention on principal points and facilitating discussion.

#### I. THE NEED FOR STUDY OF PEST RELATIONSHIPS WITH WILD AND CULTIVATED HOST PLANTS.

It is held that observations on a pest of cotton should extend over a wide field of the cultivated and other host plants of the pest. The observation of a pest's activity in cotton alone, especially a restricted area of it, is frequently misleading and unless the field of enquiry is widened, the investigator is confronted with a series of conflicting and confusing experiences which provide little or no valuable information on field conditions.

The pest situation in cotton, year to year, so depends on the influences exerted by other hosts that we require to study the relationships with other hosts both extensively and intensively. In our experience it appears that until this is done in the cotton-growing areas we shall be unable to state the real problems involved.

## II. CONTINUITY OF OBSERVATIONS.

Granted the value of intensive observations over the above field, these should be frequent and according to a regulated system. Infrequent observations of pest activity and related data, such as the presence or absence of a parasite in an isolated sample drawn from an unknown population, may give a clue to some situation, but are generally most uninformative as they are unrelated to anything approaching a continuous history.

## III. THE NATURE OF THE INFORMATION OBTAINABLE FROM INTENSIVE, CONTINUOUS OBSERVATIONS (OR SURVEYS) OF PEST ACTIVITY.

### BOLLWORM :—(A) *The Course of Oviposition.*

This is readily procurable and is the best indication in point of magnitude, duration and time of occurrence in the life of a host plant upon which to study the pest—host relationship. This is so because the high mortality amongst larvae, which differs in different host crops, makes computations and comparisons of the incidence of bollworm in crops most inaccurate.

The course of oviposition serves well to indicate moth activity and the relative attractiveness of hosts, which we find to be a highly important matter when considering the situation in cotton.

### (B) *Samples from known Egg Populations.*

Opportunity is offered during the recording of egg populations to draw frequent samples from them, at certain time intervals, for incubation and observations on the presence and identity of egg parasites. This allows the construction of a continuous record of egg parasitism by different parasites over a wide range of hosts, together with various important data on the parasites themselves.

The presence and activity of egg predators also comes under close observation.

### (C) *Larval Populations.*

These may be studied by regularized counts and analyses by instars, providing data on larval survival per population and hence on comparative mortality.



Samples may be drawn from populations for observation on the presence and identity of parasites and the instars in which they occur. This information, to be of real value, requires to be stated in relation to a known population of larvae.

Moth emergence data are also procurable—as a fraction of the late instar material in weekly samples of larvae may be utilized for pupation and emergence studies; variations in the larval populations being reflected in the samples drawn, the course of these events should approximate closely to field movements.

#### IV. THE CONDITION OF THE HOST.

Records of the growth, fruiting and other conditions of a host require to be taken and viewed in relation to the time of appearance and duration of pest activity.

Data on these subjects are matters of primary importance involving the questions of host succession, host selection, breeding areas, etc.

Measurement of temperature, humidity and evaporation in the crop.

#### V. COTTON STAINERS AND INTERNAL BOLL DISEASE.

The field of observation.

Investigations of the wild hosts of *Dysdercus* and soil-plant associations of these.

The application of continuous recording at selected points.

#### VI. THE NEEDS AND ORGANIZATION OF A SYSTEM OF ROUTINE OBSERVATION AND RECORDING.

The field staff—training of natives under European supervision.

The selection of “situations” for study.

Sampling procedure.

Technique and equipment.

The supply of quantitative data.

The practicability of survey work of this kind.

#### VII. SOME RESULTS OBTAINED IN SOUTHERN AFRICA.

## INVESTIGATIONS ON INSECT PESTS—BARBERTON.

[*Extract from Reports from Experiment Stations, 1932-33*]

Investigations by members of the Corporation's staff on the control of cotton insect pests commenced at Barberton in 1929-30. Since then the trend and progress of this branch of the work at the Cotton Station have been stated in the form of summaries only in the annual volume of Reports from the Corporation's Experiment Stations. A full report on the work of the past three years has been prepared and it is hoped that this will be issued in the form of a separate publication. The present summaries have been composed from information given in the full report.

## INVESTIGATIONS ON THE AMERICAN AND THE RED BOLLWORMS OF COTTON.

BY

F. S. PARSONS AND G. C. ULLYETT.

From the beginning of the work on bollworms at Barberton much attention has been given to a study of indigenous parasites, particularly of the egg parasite, *Trichogramma lutea*. This and other parasites have been bred in large numbers and tested extensively for their value in crops. Concurrently with this work it has been necessary to ascertain the course of bollworm infestations and the extent of parasitism occurring naturally in varying egg and larval populations throughout the seasons of the year. This information was essential, if only for purposes of comparison, on the occasions when parasites were released for trial. In this connection, however, many important factors required consideration, such as the duration of egg-laying by bollworm moths on different crops; the particular condition of plant growth, if any, associated with oviposition; the relative attractiveness of hosts, and other matters which may be termed bollworm-host plant relationships. All these relationships required to be studied and in order to obtain adequate information on them it was necessary to introduce a system of continuous observations and recording. Although this work was organised primarily to provide information considered necessary to the conduct of parasite investigations, all the data that have been accumulated are really essential to field investigations on bollworm control as they served to state the problems.

Since the inception of systematized and quantitative recording in the field, information has been obtained on bollworm-host plant

relationships which indicates feasible methods of crop management whereby American bollworm may be diverted from cotton. The early information on this subject was important and warranted considerable expansion of this branch of the studies on bollworm, and an additional result has been the inauguration of co-ordinated investigations and methods of recording between members of the Corporation's staff in the Union, Swaziland and the Rhodesias.

### I. *Methods of Field and Laboratory Survey—Bollworm.*

Methods have been devised which provide for the accumulation on standing crops of continuous quantitative data under the following general heads.

1. The course of oviposition in point of duration and time of occurrence in the life of a host crop. The observed course of these events serves well to indicate moth activity and the relative attractiveness of hosts.
2. The identity and activity of egg and larval parasites under natural conditions.
3. The identity and activity of insects predatory on the eggs and larvæ, as occurring in the varied insect communities associated with different crops.
4. Comparative mortality of larvæ in the different host crops where oviposition has been recorded.
5. Observations on growth and fruiting of host plants in relation to both egg and larval populations.

It has been found that eggs laid by the moths of both bollworms are most unevenly distributed over a crop in correlation with certain conditions of vegetative and reproductive growth. In view of this, records must be taken in all sections of the crops under survey. The method is to mark off each field into numerous blocks of convenient size and take a number of sample plants in each, upon which to record and collect eggs and larval material.

Eggs are counted twice weekly. In routine work a sample of these is collected twice weekly for incubation and observations on the presence and identity of parasites; the remainder are brushed off. If experimental releases of an egg parasite are in progress in a crop, however, eggs are marked *in situ* in order to obtain data on the fate of egg populations as they occur on the plant. Satisfactory methods of marking eggs and differentiating between oviposition periods have been devised and applied.

Larval populations are recorded at weekly intervals and samples drawn from them for observations on parasitism, growth development and pupation behaviour, under methods of treatment which simulate natural conditions closely.

All the survey work has been placed upon a routine basis. Continuous and intensive recording of the kind undertaken requires the services of a relatively large staff of observers. In our case these are supplied by natives trained to do the work under European supervision. It may be accepted that trustworthy results are supplied by the class of observer obtainable, when supervised properly, and natives undoubtedly withstand the prolonged tedium of such work infinitely better than do Europeans.

It has been possible by adhering to a calendar of operations, to record bollworm material, as indicated, over the Cotton Station and seven farms in the district throughout the past year.

At the present time at Barberton, the authors, five European assistants and seven natives are engaged on bollworm survey work. Three of the European assistants are receiving training before being posted to do similar work in Swaziland and Natal.

Some of the results from these preliminary surveys are referred to later.

## II. *The Course of American Bollworm Activity on Rain-Grown Cotton, Maize and Other Crops.*

Cotton and maize were the principal crops in growth, but the survey included numbers of others which are hosts of the bollworm.

Oviposition was found to be very closely associated with the period of bud and flower production. The duration of egg-laying thus varied according to the flowering habit of host plants.

In the case of maize, oviposition in quantity commences when the tassels (staminate flowers) are extruding and the peak of oviposition is past before silks (pistillate flowers) are abundant. This information has been obtained from intensive observations on some 20 maize crops of widely differing variety, planting dates and growth conditions.

Oviposition on maize here lasts for two to three weeks only. This is at variance with the widely accepted view that the plant is oviposited on during much of its life and most heavily during the silking period.

Numerous instances came under observation where maize in tassel was laid upon by the American bollworm moth to the practical exclusion of neighbouring cotton in freely fruiting and healthy condition. On

several farms, including the Cotton Station, three and more commercial crops of maize came into tassel in timely succession and each of these was oviposited on in varying degree during the period of tassellation. whereas no eggs or, at the most, very few were to be found on the cotton while maize in tassel existed. On other farms, where only one or two plantings of maize had been made, it was observed that the oviposition on cotton ceased when neighbouring maize plants came into tassel and eggs did not occur, except rarely, on cotton again until the oviposition of maize fell off. Several very striking examples of this course of events came under our observation.

The attraction to the American bollworm moth of maize in tassel was demonstrated clearly in both routine and experimental work and possibilities of exploiting this relationship and diverting the activities of American bollworm from cotton are being investigated. Sunflower, groundnut and bean crops may also be of considerable value in this connection.

Although the use of maize for trap crop purposes in cotton-growing countries has been advised from time to time, the practice has also been discountenanced (Entomological Conference of the Transcaucasian Cotton Committee, 1929), because it was held that the moths bred in maize would increase later infestations in cotton. Our evidence from larval surveys in both crops is that the percentage survival of American bollworm is greater in cotton than in maize. As will be mentioned in the section on the presence of American bollworm during the cool season on irrigated crops, the evidence is that moths bred there enter rain-grown crops present later on the same and contiguous farms. Where no maize was in growth last year they oviposited on cotton as the records show. In these cases, according to the evidence of our larval surveys, more moths would be bred in cotton lands per equal population of eggs than resulted in the instances when maize was present in tassel and received the attention of moths. If a timely succession of maize and possibly other rotation crops exists on the same farm, and eggs are laid on them rather than on cotton there would, on the basis of comparative larval survival, be a general reduction of bollworm. An argument against the above may be that the moths are stimulated by maize to lay more of their eggs thereon than they would be stimulated to lay on cotton in the absence of maize. At death there are still numbers of developed and undeveloped eggs in the ovarioles and oviduct which might be laid under special stimuli. Such stimuli may arise during that part of the life of the maize plant which is associated with oviposition. We have no information as yet on this point.



### III. *The Course of American Bollworm Activity on Winter-Irrigated Crops and Citrus Orchards.*

The methods of survey referred to have been applied to a study of the bollworm in these crops, of which there are relatively large acreages.

The evidence from the past season's intensive work is that the bollworm breeds extensively in vegetable crops, particularly a month or two prior to rain-grown cotton and maize. The percentage survival of larvæ is very high owing to the practical absence of insect agencies which impose checks on the larval population; and a large emergence of moths takes place in these lands when rain-grown crops are again in evidence to furnish a continuation of host plants.

In the present year oviposition on the citrus observed ended by October 7th, the latest egg-laying being on Valencia orange trees. Larvæ bred on these trees and pupating in October may be expected to give rise to moths late in November. It is possible that early summer crops would be laid on by these moths as well as vegetable crops still in growth. From information obtained in other years, however, it is known that the blossoming time of citrus varies considerably. In some years blossoming may not occur until late October. Moths derived from eggs laid on citrus then would be in flight at the end of December. Most of these moths would come from orchards of Valencia trees where the latest breeding of bollworm on citrus takes place. We are indebted to the officials at the Government Sub-Tropical Experiment Station, Nelspruit, Transvaal, for the information that there are approximately 800,000 citrus trees in this and neighbouring areas; of these approximately 400,000 are Valencias. It is conceivable that circumstances may arise which cause citrus trees to blossom unusually late in a season favouring the early development of rain-grown crops. The late emergence of moths from such extensive breeding grounds could then occasion an "epidemic" infestation of American bollworm on cotton, such as we have experienced in the De Kaap Valley on former occasions. Future survey work should afford exact information on this point.

### IV. *Larval Parasites of the American Bollworm.*

It has been determined from a study of larval parasitism over three years that a Tachinid fly, *Sturmia inconspicua*, Mg., is the only indigenous parasite of importance. This fly does not, however, parasitise young larvæ. There is, in our experience, a relative scarcity of insects parasitic on the larval stage and an absence of any which parasitise the early growth stages of the worm. Attention should be given, therefore, to the advisability of importing parasites under authority from the Government of the Union.

Breeding and experimental releases of one larval parasite, *Microbracon brevicornis*, have been carried out. This parasite occurred in association with the bollworm on certain ornamental flowering plants here, but has been recorded by one of us (Ullyett) in field crops of maize.

According to experimental evidence, which will be published later, it has been determined that larvæ parasitised by *M. brevicornis* in the field decompose very rapidly under the conditions of high humidity and temperatures obtaining at larval sites on the plant during the summer months. Decomposition is generally too rapid to permit development of the parasite larvæ and hence multiplication is minimised.

#### INVESTIGATIONS ON *Trichogramma lutea*, GIRAULT.

*Trichogramma lutea* is an indigenous parasite of the eggs of the American and red bollworms of cotton. When work on the control of cotton pests was undertaken by members of the Corporation's staff at Barberton it was decided to investigate the possibilities of exploiting this parasite.

Provision was made in 1930 to breed *T. lutea* in large numbers in eggs of the common grain moth, *Sitotroga cerealella*, according to technique devised by Flanders.\* Mass production proceeded satisfactorily under the methods employed, although we encountered strains of the parasite which would not breed in the eggs of the grain moth. Sufficient numbers of *Trichogramma* were bred in the first year of the work wherewith to conduct preliminary experimental releases in crops of cotton. These early investigations served mainly to indicate the nature of the observations required in the field in order to assess the value of released *Trichogramma*.

It was realised early in the course of this work that the history of individual eggs occurring on plants required to be known, otherwise parasitism could not be estimated with a reasonable degree of accuracy. Some of the reasons leading to this decision are as follows:—Any sample of undated bollworm eggs collected from plants contains eggs ranging in age from those several days old and about to hatch, to those laid over night, with consequent differences in the length of time they were exposed to parasitism. A collection of eggs which are mainly 48–60 hours old may be parasitised to the extent of 70%, whereas a collection composed largely of eggs deposited over night may be parasitised to the extent of 5% only. Furthermore, the eggs are

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\* Flanders S.E. "Biological Control of the Codling Moth." J. Econ. Ent. XX. 644. "Mass Production of Egg Parasites of the Genus *Trichogramma*," Hilgardia, Vol. 4, No. 16, June 1930. Calif. Agr. Exp. Stn.

destroyed *in situ* in very considerable quantities by *Orius* sp. and other predators and agencies. It is of course the surviving egg population which contributes larvæ, and it is the parasitism of these surviving or effective eggs that it is important to know in evaluating the usefulness of an egg parasite.

Particulars of the manner in which eggs are marked *in situ* on host plants are omitted in the present discussion. These methods were evolved largely to meet the needs of studying the course of egg parasitism by *Trichogramma* in release experiments. They enable data to be obtained in the crop on the parasitism of the effective fraction of the egg population.

In this work the fields of crop utilised for release purposes were marked off into blocks of  $\frac{1}{4}$ -acre and a sample of record plants was taken in each block, the sample being commonly sixteen plants per  $\frac{1}{4}$ -acre. This procedure enables parasitism to be followed over areas of light to heavy oviposition, which, as previously stated, occur in practically every crop.

The larval population was estimated weekly in each block according to the method of head capsule width (Dyer's law, 1890), and the percentage survival of larvæ computed. The data for larval population in blocks where the parasitism of effective eggs differs, furnish information on the significance of percentage parasitism of eggs.

In our experiments, up to the present *T. lutea* has been released on the wing, following emergence and fertilization in breeding containers. Releases were made in the centre of small fields or from several points in the middle of large fields.

*Release Experiments in Rain-Grown Cotton and Maize.*—The intensive methods of recording referred to above were applied in the first instance to field investigations on *T. lutea* during the season 1931–32. The results of that season's work in this connection were indicated rather fully in the summary contained in the volume of reports, 1931–32. The chief observations made were :—(1) that *Trichogramma* released in the summer months appeared to disperse in both crops more widely and rapidly than anticipated and (2) that oviposition on maize was of much shorter duration than was expected (this was before the field survey methods had been applied to a study of bollworm-host plant relationships). Owing to the short period of oviposition on maize it was not possible to carry out fully the release experiments which had been planned.

Last year's summary contained preliminary details of a release experiment in maize which was put into execution at the end of the winter. This experiment was still in progress when that summary was prepared, containing a statement that an egg parasitism of 74% had been recorded up to the time of writing. The final percentage parasitism in this experiment was 80%. This did not result, however, as forecast in the report, in a decided reduction in the numbers of larvæ compared with conditions obtaining in control areas of relatively low egg parasitism. On the contrary, analysis on proportionate grounds of the data for larval survival in the experimental and control areas showed that the percentage survival was practically identical in all cases.

In view of the dispersion behaviour observed in last year's experiments, larger areas of crop were chosen for releases in the season 1932-33. Provision was made also to release much larger numbers of parasites per unit area in an endeavour to bring about a *continuously* high parasitism. In former experiments parasitism, at the most, increased only gradually, if oviposition was sustained. It was generally the case, however, that a large proportion of the total eggs laid, often more than 50%, hatched before parasitism rose above 30%.

There were three main experiments in 1932-33. The principal results from these are discussed briefly below.

*Experiment No. 1—Releases in Maize.*—This experiment was conducted on a farm near Barberton in a fifteen-acre field of maize. The field was marked off into 60 blocks of an area of  $\frac{1}{4}$ -acre each. Parasites were released in the middle of the field twice weekly at the rate of 9,000 per acre per week. This is double the number used previously.

Concurrently with the collection of data in this experiment, records were being taken on the course of natural parasitism in maize and other crops at various points in the neighbourhood. The highest figure for natural parasitism recorded anywhere in the district during the period (December to January) was 29%. This occurred in a field of maize some miles away; otherwise the figures ranged from 7% to 13%. No parasitised eggs were found on any other crops or other host plants on the farm prior to releases. None of the earliest eggs found on the crop recorded were parasitised before parasites were released. It was assumed from these observations that when the experiment commenced parasitism due to naturally bred *Trichogramma* was non-existent or negligible.

As parasitism was being recorded in all parts of the field at once, it was possible to determine when, and to what extent, it occurred in portions of the field well removed from the points of release. These records supplied information on dispersion behaviour.

In brief, it may be said that dispersion was very rapid and extensive, so much so, that the parasites travelled to the confines of the field in less than 24 hours after the first release was made. There can be little doubt also that large numbers left the field altogether, as the parasitism of eggs on plants in the marginal area of a neighbouring field of maize rose from nil to 40% in the week following the initial two releases in the experimental area. There was abundant evidence that the dispersal of parasites released on subsequent dates was equally rapid and widespread.

The chief value, for our purposes, of releasing laboratory-bred *Trichogramma* in field crops would be to benefit from the massed activity of the parasite at desired points. As the strains of *T. lutea* used in our work up to the present disperse so rapidly, sustained mass action is definitely not obtained.

Apart from the above, it was thought at that time that oviposition on maize endured for a comparatively lengthy period, and consequently the natural increase of *Trichogramma* from eggs parasitised by released parasites, would add very materially to the parasite population. Experience in the previous year, however, had been otherwise and attention was being given to the course of egg-laying by American bollworm moths on various crops. In the present instance oviposition was in progress for only 18 days, and, of this time, was extensive for little more than a week. This, as has been found out since, is the typical course of oviposition on maize here. The developmental period of *Trichogramma* in the egg in the field is 7-9 days during the summer and hence in this case only one generation of the progeny of the released parasites could have emerged in time to increase the population while parasitizable eggs were still present in numbers.

Despite the foregoing the percentage parasitism of the eggs in many blocks of the field, not necessarily those where releases were made, ranged from 60-80%.

Percentages of survival of larvæ under the different conditions of egg parasitism in this experiment have been computed. The differences between these were of no consequence for practical purposes.

*Experiment No. 2—Releases in Cotton.*—The releases were made in the centre of a field of cotton of about 23 acres in extent. Parasites were released twice weekly at the rate of 9,000 parasites per acre per



week. Releases were made over a period of a month. Records were taken in the central 4 acres only. Although a moderately heavy oviposition was in progress, parasitism of American bollworm eggs never exceeded 12% and of red bollworm eggs 2%.

*Experiment No. 3.—Releases on the Cotton Station, Barberton.*—*Trichogramma* was released weekly at the rate of 5,000 per acre per week in all crops where egg-laying by American bollworm moths was in progress. Parasitism of both American and red bollworm eggs in cotton was negligible. In maize it attained 40% in one case, but was generally less than 30%.

*Releases of T. lutea in Maize and Cotton at Gatooma, Southern Rhodesia.*—These experiments were organised and carried out in co-operation with J. E. Peat and co-workers at Gatooma during the past season. They were essentially the same as the experiments at Barberton as discussed above. The parasites released at Gatooma were bred at Barberton for the purpose.

Low percentages of parasitism were recorded throughout the periods of observation.

In view of the results from the Experimental releases in rain-grown crops, as above, it has been decided to discontinue breeding and field trials of the strains of *Trichogramma* with which we have been working.

Recently a strain of *Trichogramma* was procured by the senior author when at Gatooma. According to records on natural parasitism in maize and cotton, which have been accumulated by Peat in Southern Rhodesia, this strain promises to be of considerably greater value than any that we have procured from wild stock in the Union.

The Rhodesian strain is being multiplied at Barberton and at the present time is being released experimentally in irrigated vegetable crops. It is proposed to utilise it later in the year for trials in fields of cotton.

The results of these investigations and various other data which have been obtained here in the course of work on *Trichogramma* will be published separately later.

#### INVESTIGATIONS ON THE RED BOLLWORM.

*Exposure and Destruction of Pupae by means of Farm Implements.*—Preliminary work on this subject was rather fully summarised in the Report for 1930–31. Since then attention has been given during one season (1932) to a study of the position in the soil of red bollworm pupae as they occur naturally in fields of growing cotton. Investigations

covered different soil and growth conditions and afforded valuable information on the location of pupae within the portion of the inter-row area covered by cultivating implements.

Owing to the cessation of operations on the estate where the work was being carried out the data are incomplete and it has been necessary to postpone the investigations until arrangements can be made elsewhere where red bollworm is more plentiful than at Barberton.

*The Utilization of Small Areas of Ratooned Cotton for Purposes of Attracting Red Bollworm Moths.*—In order to provide the trap, cotton plants must be ratooned or allowed to stand over after picking is completed. Whether ratooned or stand-over, fruit will, in all probability, be in evidence on the plants sometime in the winter months. Unless this is removed and destroyed at appropriate times, after oviposition commences in the winter months, larvae will proceed to pupation. According to recent records the pupal periods of these are frequently of sufficient duration to mean that moths may not emerge until January, after the trap has ceased to attract moths. Formerly it was thought that pupae formed from the early larvae on trap plants would all give rise to moths before plant cotton was in bud.

Recent records have shown that the percentage survival of red bollworm larvae in the dry season months is much higher than it is in the Summer, due no doubt to the comparative absence of insect agencies serving to reduce their numbers. It may be expected in consequence that a relatively large proportion will pupate. The accumulated pupae formed over August and September in a warm winter season might well give rise to as many moths as, or even more than, would emerge in cotton lands from the over-wintering pupae themselves, as evidence points to a heavy mortality amongst the latter in the field.

Prior to the recent adoption of more frequent examinations, the view was held that the numbers of larvae bred in trap areas in mid-winter were negligibly few. This is found not to be the case and fruit must be collected off the trap plants and destroyed every 3 to 4 weeks after egg-laying commences. This involves constant observation to determine when egg-laying does begin. It is doubtful if the necessary measures would be taken in general practice by cotton farmers and the method of trapping as a means of red bollworm control is being discouraged.

*The Variable Incidence of Red Bollworm.*—Of late years the annual infestation has diminished greatly in some districts, but not in others.

These variations are attributed mainly to (i) The presence or otherwise of ratooned cotton, particularly in a mild winter. Under favourable circumstances ratooned cotton offers every opportunity for the multiplication of red bollworm, and plant cotton in the neighbourhood may be heavily attacked towards the end of the season by bollworm bred in the ratoons. Instances of this have been recorded. The acreages under ratooned cotton are greatly diminished in some areas, less so in others, and the severity of attack by red bollworm appears to be related to the amount of ratooned cotton present. (ii) Seasons of very late planting have been experienced in recent years. According to records, moth emergences from over-wintering pupae are practically complete by the end of December and in the absence of ratoons late planted cotton avoids the great majority of these moths, as events have indicated.

#### INVESTIGATIONS ON COTTON STAINERS AND INTERNAL BOLL DISEASE

BY

E. O. PEARSON.

Investigations on this problem have been in progress at Barberton since the season of 1931-32. During that and the following season Mr. Parsons carried out observations on the migration of stainers to the crop and the damage resulting from that migration, on some of the wild host plants of the stainer and on possible methods of trapping the stainer in the crop. Since November, 1931, it has been possible for the writer to carry on whole-time work upon the problem.

Investigations have primarily been directed to cotton stainers. Following the experience gained from different methods of estimating the stainer population on the Cotton Experiment Station in 1931 and 1932, a uniform system of recording populations by sampling 10-acre blocks was employed in 1933 on the Cotton Experiment Station and four farms in the Barberton district, and also, in collaboration with Mr. Lochrie and Mr. Bowmaker, on three farms in Swaziland and the Experiment Station at Magut, Natal. The records so obtained show that in the absence of adjacent ratoon or stand-over cotton, stainers appear in plant cotton at the end of February or the beginning of March, the migration reaching a peak about a month later. *Dysdercus nigrofasciatus* appears earlier than either *D. fasciatus* or *D. intermedius*. The last named species is present in small numbers only throughout the season. *D. nigrofasciatus* and *D. fasciatus* are very variable in relative abundance. In 1931 and 1932 *D. fasciatus* appeared in large numbers and bred up a very large population in the crop. *D. nigrofasciatus*

appeared in about equal numbers, but the natural increase was very much smaller than in the former species. In 1933 *D. fasciatus* was practically absent from all points save those which were in the vicinity of adjacent ratoon or stand-over cotton.

The stainer population varied as a whole from farm to farm, according to the condition of the crop. Detailed studies of the distribution of stainers within a single 10-acre block and comparisons of the population on coarse strains of U/4, *e.g.*, 9264 and 913, with that on finer varieties, *e.g.*, 9312, indicated that population density varied with the amount of cover available rather than with the amount of crop matured.

Where the normal migration occurs, all three species of stainer pass through two and a partial third generation on the crop. At the end of the season, when the crop is cleaned up, some of the adults migrate from it and the remainder, together with the great majority of the nymphs, can be destroyed. Clean-up measures on the Cotton Experiment Station at Barberton, in which the cotton is cut out, stacked and these stacks shortly afterwards burnt, the land being ploughed as soon afterwards as possible, have been very successful. Trapping on seed heaps followed by destruction of the catch by periodical sprayings with soap emulsion have been shown by Mr. Parsons to be effective and this measure might conveniently be used where cotton is being ratooned or allowed to stand over for cattle grazing.

An explanation of this annual flow and ebb of stainers in the crop has been sought in observations on the wild host plants of the stainer, which comprise certain genera belonging to the families *Malvaceæ*, *Sterculiaceæ* and *Bombacaceæ*. Extensive surveys have been made throughout the Transvaal and rapid tours of portions of Swaziland, Zululand and Portuguese East Africa to determine the distribution of these host plants, while more intensive observations have been made on the fruiting habits and seasonal infestation by stainers of those which occur in the Barberton district. These hosts fall into two groups, firstly, herbaceous and shrubby species, secondly, arboreal species.

The first group consists of the genera *Abutilon*, *Cienfugosia*, *Gossypium*, *Hibiscus*, *Pavonia*, *Sida* and *Thespesia* in the *Malvaceæ* and *Hermannia*, *Melhania* and *Waltheria* in the *Sterculiaceæ*. A number of these species are comparatively rare, but there are several which are widespread, abundant and gregarious and on which stainers have been found breeding. The flowering and fruiting of these species are closely dependent on the rains and are consequently rather irregular. Taking the region as a whole, there is probably a continuous supply of

fruit in all stages between October and May. Winter rains may produce a small out-of-season crop, though in the dry months the majority of the colonies die back completely save for those in sheltered, moist situations, where flowering and fruiting may be continuous throughout the year. *D. nigrofasciatus* is the only species regularly found on these plants: the other two species have been taken infrequently on *Gossypium* and *Hibiscus* spp.

The second and by far the more important group consists of the Baobab, *Adansonia digitata* L., and two species of *Sterculia*. The Baobab occurs in great numbers in the low veld of the Limpopo basin and extends southwards in diminishing density through the North Eastern Transvaal, petering out just south of the Olifants River. It occurs in all types of situations but is densest on low ridges and foothills of the mountains.

It flowers during the summer and the fruits ripen off and shed during the following August and September, when they constitute the chief food supply of baboons, which appear to be the chief agency in distributing the seed and in rendering it available for insects. In detailed searches made in March, 1932, and September, 1933, no stainers were found associated with these trees.

*Sterculia Rogersii* N.E.Br. is confined to the Low Veld and is limited within that area to two sub-regions: (a) Outcrops of metamorphic rock forming long ridges which may break up into strings of kopjes or isolated bosses, mainly running from east to west. The extremely steep northern slopes of these outcrops, on account of the resultant exposure and broken rock masses, support a scanty xerophytic or strongly deciduous vegetation, frequently associated with Euphorbias and differing widely from that of the surrounding bushveld. (b) Much dissected country towards rivers, or low ranges of hills determining the course of the latter, subject to considerable wash exposing surface rock. The vegetation here is similar to that of the surrounding country, but added character is given to it by variations in exposure and drainage. With its enlarged, water-storing trunk, spreading roots, inconspicuous foliage and strongly deciduous habit, *S. Rogersii* is well adapted to such habitats and frequently forms large and extensive colonies where they occur. Flowering in August-September, the main crop of fruit splits open on the trees from late October onwards, though early or discontinuous rains may induce a number of partial crops. By February the seed has shed and rapidly disappears from below the trees, being a favoured food of small rodents.



The following records of stainer infestation on these trees have been made in the Barberton district :—

- |       |                        |                                                                                                                                             |
|-------|------------------------|---------------------------------------------------------------------------------------------------------------------------------------------|
| 1930. | November.              | Stainers (? spp.) fighting in to trees.                                                                                                     |
| 1931. | Mid-November.          | <i>D. nigrofasciatus</i> and <i>D. intermedius</i> fighting in.                                                                             |
|       | Late November.         | <i>D. fasciatus</i> fighting in.                                                                                                            |
| 1932. | January.               | First generation of adults of all species, <i>D. fasciatus</i> and <i>D. nigrofasciatus</i> in equal numbers, <i>D. intermedius</i> scarce. |
|       | February.              | Numbers of adults decreasing, second generation present as young nymphs.                                                                    |
|       | March.                 | Stainers all gone.                                                                                                                          |
|       | Late October–November. | <i>D. intermedius</i> migrating to successively opening colonies.                                                                           |
| 1933. | January.               | Small numbers of late instars of <i>D. intermedius</i> .                                                                                    |
|       | February.              | Stainers all gone.                                                                                                                          |
|       | Early November.        | <i>D. nigrofasciatus</i> and <i>D. intermedius</i> fighting in, the former predominating.                                                   |

These infestations are of the same character as those which subsequently occurred on cotton, and it is probable that trees of this species are the immediate source of the stainer infestation in cotton in the district.

*Sterculia murex* Hemsl. is confined to the so-called middle veld, the narrow strip of foothills and spurs to the east of the Drakensberg and Zoutpansberg between 2,500 feet and 3,500 feet, which receives its character from the fact that the mountain escarpment precipitates a higher rainfall (up to 40 inches) over this area than occurs in country immediately to the east. The vegetation might be described as orchard bush. Within this limited area the species is fairly prominent, occurring in small groups of quite large trees. It flowers in August–September, but the large and woody fruits do not dehisce until the following May. The leaves, which persist from spring to spring, are large and digitate, forming, when shed, a thick carpet below the trees. These characters, together with the rather high rain and dew fall of the region in which the trees occur, suggest that the species may be an important factor in the over-wintering of the stainer. *D. nigrofasciatus* has been found breeding below the trees in September and February and according to local information stainers migrate to them from the lands at the conclusion of the cotton crop, when the fruits are dehiscing. It is uncertain, however, whether the species is sufficiently numerous or widespread to be of more than local importance.

To sum up, it is difficult at this stage to state with certainty the relation of the three species of stainer to their wild host plants. *D. nigrofasciatus* appears to be the most catholic in its tastes and the normal infestation in cotton is probably derived from the over-wintering of an exiguous population on herbaceous hosts in sheltered habitats, reinforced by mass breeding on these and on *Sterculia Rogersii* early in the summer. *D. intermedius* is rare in South Africa, which is situated on the fringe of its known distribution. It is infrequent on wild hosts other than *S. Rogersii*, on which it commonly occurs in Portuguese East Africa and Nyasaland. *D. fasciatus* does not favour herbaceous hosts and apparently depends in this country on stand-over cotton or a large and late crop on *S. Rogersii* as a take-off for plant cotton. Its over-wintering habits are problematical, but its common association with the baobab in other parts of Africa suggests that its relationship with this species in the Northern Transvaal requires fuller investigation.

In conjunction with population records in the crop, weekly systematic records of damage to the crop have been obtained by collecting samples of splitting bolls, which have then been examined for puncturing, and graded for degree of staining. A very high degree of correlation between number of punctures per boll and percentage staining has been demonstrated. It is, however, difficult to trace any clear relation between the curves for stainer population and those for stainer activity, as measured by puncturing or percentage staining. This is due partly to the fact that estimates were made on split bolls and thus represented a summation of the damage which had been done to the bolls throughout their life, and partly to the fact that population records for adults do not truly represent the population of effective vectors of disease, since late instars as well as adults can cause infection. The fact has also to be taken into consideration that decreasing temperatures towards the close of the season differentially affect the maturation period of bolls, the growth rate of nymphs and the rate of spread of infection within the boll, thus still further distorting any relationship between the different curves. Estimates of damage based on the examination of green bolls gives a better correlation, but even so there are a number of discrepancies which can only be explained by assuming that under certain conditions stainers vary in their effectiveness in spreading disease.

In connection with the question of transmission of disease by nymphs, experiments have been made in which nymphs of different ages have been fed on bolls of different sizes. These preliminary investigations have led to the conclusion that nymphs which have not reached the fourth instar are not efficient vectors of disease, owing

to the mechanical difficulty of obtaining food from bolls sufficiently advanced to provide proper nutrition.

It is, however, possible for nymphs in the early stages to survive on a diet of unopened bolls, but with heavy mortality and considerable prolongation of the stadia in those which survive.

In practice, should eggs be laid in the field before open bolls are available as food, the green bolls, by their very retarding action, would suffice to tide over a small population until open bolls were available.

In estimating damage due to staining in the manner described, no differentiation has been attempted between that due to the true fungi of Internal Boll Disease, *Nematospora* spp., and that due to other, largely bacterial, agencies. Without microscopical examination, impracticable on a large scale, it has not been found possible to distinguish the effects of different organisms causing staining.

That other organisms may play an important part was first noticed in the ratoon crop of 1931, when in the absence of stainers a considerable degree of staining occurred, which was correlated with the presence of a number of other Hemiptera, largely Pentatomidae, of which the commonest was the small *Piezodorus purus* Stål. Microscopical examination of diseased bolls, the experimental transmission of disease by exposing sterile bolls to the insects suspected and the puncturing of sterile bolls with sterile needles and the subsequent exposure of these bolls to wind-borne infection, led to the belief that in the early part of the season, when conditions are more humid, staining is caused to a considerable extent by bacterial infection present on the surface of the boll and entering it through insect punctures.

Later in the season the infection in the crop is more definitely due to *Nematospora*. Isolations of both *Nematospora gossypii*, Ashby and Nowell, and *Nematospora coryli*, Pegl, have been made from cotton bolls in the past season, but the former is more abundant than the latter. By caging adult stainers collected in the field on sterile bolls it has been shewn that all three species are capable of transmitting *N. gossypii*. In addition, adults of *D. nigrofasciatus* collected from wild cotton and from *Hibiscus vitifolius* L. in November and December, and adults of *D. intermedius* collected from *Sterculia Rogersii* in November and December and from *Hibiscus* sp. in January have all been shewn to be carrying *N. gossypii*. It thus appears that the migrants to the crop are already infected with *Nematospora*.

Experimental inoculations of sterile bolls have been made with pure cultures of *Nematospora* spp., injected hypodermically in the form of spore suspensions. The course of infection has been followed both in these bolls and in those exposed to stainer puncturing. The infection spreads more rapidly in bolls which have reached the stage where rapid growth has ceased and where lint thickening is in progress than in young bolls which are still in the stage of rapid growth, or in older bolls which are beginning to mature, dry out and split. The intensity of the infection increases regularly with the incubation period of the fungus within the boll. In neither species does the staining extend beyond the loculus in which the infection starts, nor does the fungus occur within the seed except following direct puncturing of that seed. The condition known as staining is due to the coagulation and post-mortem discolouration of the protoplasmic contents of the lumen of the lint hair. This accounts for the fact that in older bolls, where vacuolation of the lint hair has occurred, staining develops to a lesser extent following infection.

The two species of *Nematospora* vary considerably in their virulence. In the early stages of infection by *N. coryli*, when the fungus occurs in the form of short hyphal lengths and immature sporangia, the region of staining is approximately coextensive with that occupied by the fungus. In the case of *N. gossypii* the influence of the fungus extends far beyond the region where it is found, which is confined to the immediate neighbourhood of the puncture, and there is extensive breakdown and matting of the lint, which is reduced eventually to a papery membrane closely adpressed to the seed surfaces, forming a kidney-shaped loculus. In both cases, in the later stages of infection the fungus appears largely in the form of free spores, sometimes singly, sometimes in groups loosely held together by their appendages, and these spores are distributed throughout the whole loculus.

The extension of the staining beyond the region occupied by the fungus suggests that the death of the lint hairs is due to a toxic substance liberated by the developing fungus. Bolls inoculated with a sterilised centrifuged suspension of a pure culture of the fungus developed staining which was indistinguishable from that produced by the living organism. This was very much more marked in the case of the toxic solution prepared from *N. gossypii* than in that from *N. coryli*, whilst control inoculations of sterile water produced no symptoms of disease. It thus appears that the fungi of stigmatomycosis may produce their effects in the manner suggested.

In introducing his paper, Mr. PARSONS reviewed briefly the circumstances leading to the inauguration of continuous surveys of cotton pests in Southern Africa with special reference to the American and red bollworms. Formerly considerable attention had been given to breeding and experimental releases of various parasites of the bollworms. The chief obstacle encountered in the evaluation of their usefulness in the field was the transitory nature of infestations, which diverged widely from expectation and experiences as reported elsewhere. In effect infestations did not persist long enough in any crop to permit tests of the efficiency of released parasites to be carried out. On comparison of accumulated records of infestations, it appeared that possibly it was only during certain phases of the insect's food plant's growth that it was attractive to the egg-laying moth, and attention was devoted to a study of the actual course of oviposition, particularly by American bollworm moths, in relation to food plant development, firstly on cotton and maize and latterly over a wide range of rain-grown and irrigated crops, orchards and weeds. As a result, bollworm-food plant relationships had been found that revealed the usual course of oviposition on different host crops, and larval populations could be dated accordingly. Mr. Parsons pointed out that had that knowledge been available before, the conduct of parasite work, if undertaken at all, would have been organised with a proper perspective.

Sir Geoffrey Evans had told the Conference in effect that after introducing certain parasites into Trinidad, they had found it necessary to study the life of the hosts, as the parasites were not doing what was expected of them. The experiences referred to afforded examples of the need to study pest relationships with wild and cultivated host plants.

In further discussion of the need for study of pest and food plant associations, it was stated that although the American bollworm was universally distributed and had been recorded on a vast number of food plants, for some of which it exhibited preferences, the available information was of vague application and it could not be assumed from it that the moth's activity was governed by response to stimuli at definable periods in the life of a host plant, nor, on the other hand, could it be assumed that its behaviour was indiscriminate; thus, in a sense, one must work always in the dark. Whether the bollworm moth did or did not behave according to any usual habits in relation to a food plant and its stage of development appeared to workers at Barberton to be questions of primary importance, and investigations on these matters seemed to be a logical and necessary prelude to field studies on bollworm control.



If field studies were to be really productive, they must be intensive as well as extensive and continuous. On the subject of continuity of observations and records, the view was put forward that these must be based on factors of both insect and food plant development, a simple example being that if the course of egg-laying on a crop were under observation and the incubation period of the egg were 4-5 days, then records must be taken at least twice-weekly on a sample of marked plants, otherwise hatching occurred and records were lost.

In surveys of bollworm activity in South Africa (and the same procedure was being followed in the Rhodesias as well as in Swaziland) studies of the pest-food plant association were based on the course of oviposition. In actual practice many different host crops were under observation at several points at the same time and the data from oviposition records taken in all of them were plotted in the form of comparative graphs, showing the duration and magnitude of egg-laying on each. In referring to the nature of the information obtainable from the surveys, examples of the graphs compiled from data taken over the summer seasons of 1933 and 1934 at Barberton were shown to the Conference during the discussion, in order to demonstrate that moth activity might be followed, under the system adopted, and that the relative attraction to the bollworm moths of host crops was a point of great importance.

Although emphasis was laid on summer records, when cotton was being grown, the activity of the American bollworm in winter-irrigated crops, citrus orchards, etc., was also followed as closely, and it was stated that up to a point it was possible to predict heavy attacks on summer crops from information obtained in the cool season, and with increasing knowledge, derived from the surveys, it might be possible to divert the greater part of American bollworm moth activity from cotton in some areas. That would be done by instituting in association with cotton appropriate plantings of other hosts more favoured by the moth, thus putting on a sounder basis the old idea of the trap crop. Mr. Parsons suggested that if enough were known about the movements and preferences of the bollworm moth, it might be possible to exploit them in systems of crop management.

After inaugurating continuous records of egg populations, opportunity should be taken to draw from them twice-weekly samples of eggs for examination for parasitism in order to learn with some accuracy what parasites were present and what part they played. After carrying out work on those lines for several years, it might be possible to say whether anything was likely to be gained by exploiting indigenous parasites, or whether it was advisable to introduce exotic kinds.

Attention was given to the degree of parasite and predator activity resulting in different insect communities associated with various crop plants of different habits of growth. Mr. Parsons considered that it did not seem possible to remove from speculation the part played by those agencies unless they were subjected to continuous prolonged study.

With reference to larval populations, the statement was made that larval survival differed very considerably according to the type of food plant.

Mr. Parsons said that in the past attention had been given mainly to the pest and far too little to the host plant. This seemed to be largely because observations on insects were insufficiently frequent and sustained to decide whether attacks were associated with particular periods of development of the food plant; also attention was given mainly to the incidence of broods of larvæ and nymphs, which were always composed of several stadia, subject to varying mortality and forming a mobile population, of which it was very difficult to find the commencement, magnitude and duration, especially with overlapping generations. In the case of bollworms, egg-seeking as the stationary, basic indicator of moth behaviour was too exacting and extensive unless a number of assistants were available, and, as they generally were not, investigations of this nature had suffered. Further measurements of food plant development, such as growth, flowering curves, and other data required to be added if we were to learn what, if any, relationships existed between cotton pests and their several host plants, and thus the part played by them in the cycle of feeding and breeding activity.

The same general ideas were held to apply to stainer investigations. Investigations were going on with a view to locating the wild hosts of the species of *Dysdercus* concerned and to following the yearly course of stainer activity on them and the migrations into cotton. To that end surveys were being made and observation posts established in parts of the Eastern Transvaal, Swaziland and Zululand in key positions for determining what the real problems were.

Mr. Parsons then discussed the use of trained European supervisors and native enumerators, and pointed out some of the difficulties of sampling procedure, a matter which presented unusual difficulties and which was under discussion with workers on statistical methods in England.

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# THE INTERRELATIONSHIP OF WILD HOST PLANTS AND COTTON WITH REFERENCE TO VARIATION IN STAINER POPULATION IN NORTHERN RHODESIA.

BY

A. G. BEBBINGTON (*Empire Cotton Growing Corporation*) AND W. ALLAN  
(*Northern Rhodesia Department of Agriculture*).

The history of the attempts to introduce cotton as a crop for the European farmer in Northern Rhodesia is one of continuous failure. The culminating failures of the seasons 1925 and 1926 were largely due to jassid, and it was thought that the substitution of resistant strains for those under cultivation might solve the problem. Although cotton of the U.4 type has proved sufficiently resistant and well suited to the climatic conditions, the losses caused by American bollworm and stainer have been such as to make the production of cotton a precarious undertaking for the European farmer. The incidence of pests has varied very considerably. In some years fairly satisfactory yields have been obtained, but in others the crop has been almost completely destroyed.

During the five years covered by the present investigations, stainer has been responsible for the greater part of the loss, and its importance as a pest in the European farming area of Northern Rhodesia is apparently considerably greater than in countries to the South.

The purpose of this paper is to outline very briefly the information which has so far been obtained on the identity and distribution of the wild hosts of *Dysdercus*, and their relation to the cycle of the pest on cotton. It is thought that some account of the work may be of value, although it is as yet incomplete.

The three species of *Dysdercus* involved in the transmission of internal bollrot are, *D. supersticiosus*, *D. fasciatus* and *D. intermedius*. It is proposed to discuss the two former species only, since *D. intermedius* occurs both on cotton and on the wild host plants in very small numbers, and is of little importance as a vector of internal bollrot.

A figure showing the annual fluctuations of populations on cotton, *Thespesia rogersii* and various species of *Hibiscus* has been compiled. It is necessary to emphasise the theoretical nature

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of this figure. It represents not the actual fluctuations in any one year, but the "normal" of a series of years. Major deviations from this distribution, which have occurred in particular seasons, will be discussed.

THE HOSTS OF *DYSDERCUS SUPERSTITIOSUS*.

The rainy season of Northern Rhodesia extends from November until March or April. *Thespesia rogersii* comes into flower after the commencement of the rains in November and *D. supersticiosus* then appears on this host. There is no doubt that the appearance on *Thespesia* is the result of a definite migration, since in each year it has been preceded by numerous records of the species taken in flight and scattered throughout the veld on many types of plant which are not normally hosts. *D. supersticiosus* does not remain long on *Thespesia*, and a complete disappearance from this host is generally recorded in January or February. In some years flowering of the trees is greatly reduced owing to bud-shedding caused by the larvæ of a Cossid moth, and in such years the stainer appears on *Thespesia* in small numbers.

From December until March, or possibly later, various early flowering species of *Hibiscus* carry a very large population. The species on which *D. supersticiosus* have been found to breed are listed :—

## Annuals :

*Hibiscus trionum* Linn.

*H. hastifolius* E. Mey.

*H. esculentus*.

*H. asper* Hook f.

## Herbaceous biennials or perennials (Hemicryptophytes) :

*H. baumii* Gürke.

*H. panduriformis* E. Mey.

*H. articulatus*.

## Shrubby perennials :

*H. dongolensis* Del.

*H. rosa-sinensis* Linn.

Of the species listed the most important host during the December-March period is apparently *Hibiscus trionum*. The

insect breeds freely on this plant, which occurs commonly on moist situations and especially on the Kafue Flats. During January and February of the present year an enormous population was bred there. Practically every plant observed on the Flats carried a large colony of nymphs. *H. baumii* and *H. panduriformis* are also favourite hosts, but are not so abundant. *H. articulatus* and *H. hastifolius* are apparently of minor importance, while only occasional plants of *H. esculentus* are met with. The shrubby *H. dongolensis* is not a favourite host, only a few specimens having been taken on it, while *H. rosa-sinensis* is merely an ornamental garden shrub.

A movement of *D. superstitiosus* from the early flowering *Hibiscus* spp. and from *Thespesia* begins in February, and the stainer appears on cotton generally when squaring is commencing. It was at first thought that this influx was associated with flowering, but it has since been found that the stainer may appear on cotton in large numbers before flowering has commenced. The influx into cotton continues and reaches a peak generally in March. This migration apparently coincides with the maturation of a generation on the early flowering hosts and with the drying up of these hosts. The later flowering host plants, of which *Hibiscus asper* and a closely related species, are by far the most important, commence flowering generally towards the end of February and a large population accumulates on these hosts. Breeding takes place freely after the setting of the first fruits, and population increases rapidly, reaching a peak in May. As the plants dry off the population declines, but breeding continues into August.

Our observations suggest strongly that *D. superstitiosus* is more attracted to *Hibiscus* than to cotton. The influx peak on cotton is followed by a surprisingly small "breeding peak" representing maximum emergence of adults of the F.1 generation. These leave the cotton, and from the middle of May onwards *D. superstitiosus* is practically never recorded on the crop, although it occurs in large numbers on closely adjacent *Hibiscus asper*. The year 1932 was remarkable, both for the very small numbers in which *D. superstitiosus* appeared in cotton, and for its abundance on *H. asper*. This plant appeared in almost pure stand over very large areas in the farming district. Unfortunately, since both writers were on leave at the time, no records of the first flowering were obtained, but the distribution of the rains suggests that it would be abnormally early. Chiaromonte has noted a similar preference for *Hibiscus* in the case of *D. cardinalis* in Italian Somaliland. (*Rev. App. Ent. Ser. A., pt. 7, July, 1932.*)



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During the months of September and October, after its disappearance from *H. asper* and before the early rains, *D. superstitiosus* has not been recorded on any host plant, except for a few records from standover cotton in the earlier part of September. Search has been carried out continuously on *Thespesia* and intermittently on Baobab (*Andansonina digitata*), *Sterculia quinqueloba* and *Sterculia triphaca* throughout this period. In view of the probable necessity for fairly humid conditions (see Whitfield, *Bull. Ent. Res.*, Vol. 24, 1933) it is thought that the species may carry over the exceedingly hot and arid months of September and October on *Hibisceæ* growing in wet situations. In this connection it may be noted that large areas on the Kafue Flats remain moist throughout these dry months, and it may be that the species exist on early flowering plants in such situations. This possibility awaits more thorough investigation.

*D. superstitiosus* has not been found at any time of the year on the two local species of *Sterculia*. Its occurrence on the fallen seeds of Baobab during November and December have been noted, but records on this host are scanty, since there are no colonies near the Station.

THE HOSTS OF *DYSDERCUS FASCIATUS*.

*D. fasciatus* breeds on the fruits of *Thespesia rogersii* and on the fallen seeds of Baobab throughout the year, but the former is by far the more important in the farming area. The Baobab, except in one isolated case, occurs only as occasional scattered trees which have never been known to carry a stainer population throughout the year since fallen fruits are rapidly removed by natives.

During September and October the population of *D. fasciatus* on *Thespesia* is relatively small. The majority of trees have shed their leaves and much or all of their crop, and breeding continues only on such trees as are still carrying a large crop of old bolls and are shaded either by neighbouring trees or by the retention of their leaves. The trees come into leaf in November and are in full foliage early in December. This is followed by a spread of the stainer from the dry period foci to other trees carrying old bolls where conditions have now become suitable for breeding. Towards the end of February or the beginning of March, food supply is rapidly diminishing owing to rotting and shedding of the old bolls and a wandering movement takes place, resulting in the appearance of the species on cotton in increasing

numbers during late March and early April. At this point the new crop set in November should be coming to maturity, and therefore a new food supply would be available. In many years, however, the larva of the Cossid moth previously mentioned destroys practically the whole of this crop, and thus movement takes place. This was well illustrated during the 1933 season when practically the whole of the new crop was allowed to come to maturity and began to open in late February. As a result, heavy breeding took place on the trees from March onwards, and the species did not appear on cotton until late June, when apparently pressure of population caused a movement. At a time when the influx into cotton is usually recorded, trees on the margin of the lands were bearing masses of stainers, while not a single specimen could be found in the cotton. *Thespesia* is capable of recovering from the loss of the early crop and a second setting of buds and flowers takes place. This second crop opens from May onwards. With the opening of large numbers of bolls the population increases rapidly on many trees, reaching a peak in August. In the latter part of August shedding of the leaves and crop begins, veld fires accelerate the loss of crop, and at this time the fruit is picked by natives. The stainers disappear from many trees and the population again becomes restricted to the dry season foci.

Apart from *Thespesia rogersii* and Baobab the only host on which *D. fasciatus* has been observed to subsist and breed is *Hibiscus dongolensis*. This shrub comes into flower with the early rains. The stainer appears on the host in December and breeds freely, but there is no evidence that it can carry a population throughout the year.

Although continuous records have not been kept on *Sterculia triphaca* and *S. quinqueloba*, since the nearest colony is fifty miles distant from the Station, observations have been made from time to time throughout the year. *D. fasciatus* has not so far been found on these two trees, although very often closely adjacent *Thespesia* has been found to carry a very large population.

Annual hosts appear to be of little importance in the cycle of *D. fasciatus*. Occasional specimens have been taken on *H. asper*, but so far the species has not been recorded on any of the other annual *Hibisceæ* listed as hosts of *D. superstitiosus*.

#### DISTRIBUTION OF STAINER HOSTS.

The investigations of which the results have been described were confined entirely to the European farming zone and the

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Mazabuka district in particular. This district consists of thorn country of the "savannah" type, in which *Acacia woodii* is the predominant tree. In such country *Thespesia rogersii* is abundant and *Hibiscus spp.* are numerous. The adjoining alluvial grasslands of the Kafue Flats also carry an abundance of annual *Hibiscus*. Conditions are therefore favourable to a maximum population of *D. superstitiosus* and *D. fasciatus*. The great majority of European farms occupy thorn country of the Mazabuka type and have the same host plants about their lands and in swampy places. For this reason stainer has always been a pest of primary importance throughout the entire farming area.

During the course of these investigations an examination of the country which is being made by the Ecological Survey has shown that the thorn belts occupied by the European farms are of a peculiar type which forms an extremely small proportion of the whole territory. Vast areas are covered with *Brachystegia-Isobertinia* or other woodland wherein *Thespesia* is restricted to ant-hills, or in some cases is absent. Outside the thorn belts *Hibiscus spp.* appear to be less numerous, and in some areas those recorded as hosts are rare or absent. Freedom from *Thespesia* and perennial hosts generally has been found to be particularly marked in the Kalahari Sand country of Barotseland.

A series of trials is now being carried out in areas outside the European farming region in collaboration with the Ecological Survey. The purpose of these trials is to ascertain the incidence of stainer in districts where known host plants are few or absent. Information from these trials is not yet available.

A sketch map is appended showing the sites of cotton trials which are being carried out in various types of country, together with the distribution of *Acacia woodii* "Savannah." A rectangle on the map encloses the European farming area, and the principal maize farms may be taken as coinciding with the *Acacia* belts within this rectangle. It is to these belts only that the results outlined in this paper are applicable.

The writers are indebted to Mr. C. G. Trapnell of the Ecological Survey for much of the information concerning the distribution of host plants throughout the Territory.

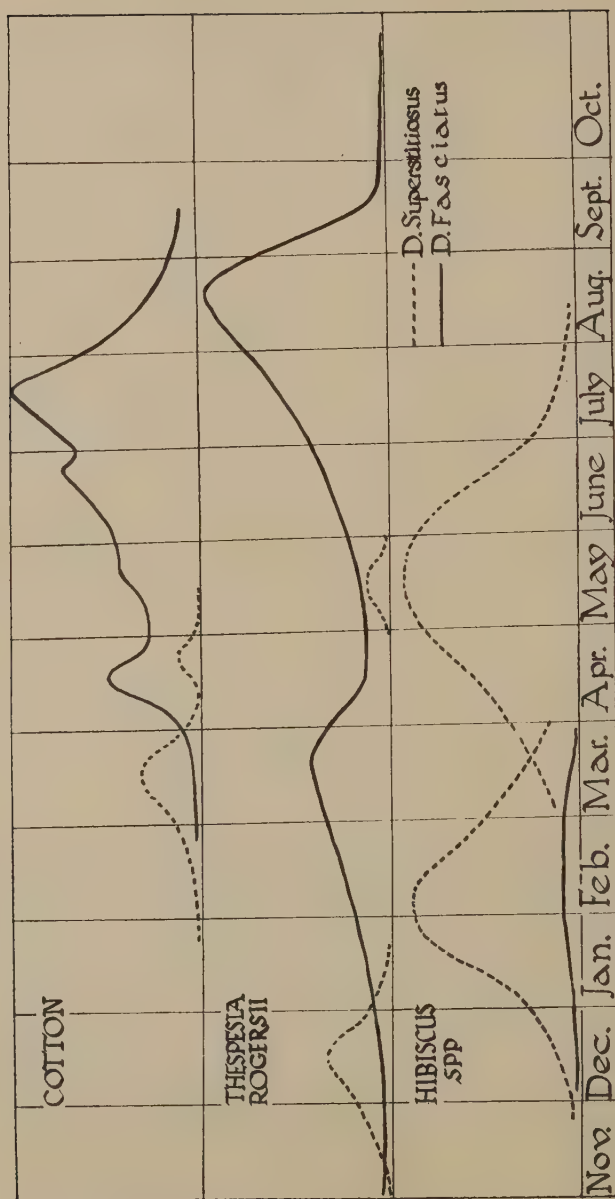


DIAGRAM SHOWING SEASONAL FLUCTUATIONS IN THE POPULATION OF TWO SPECIES OF *DYSDERCUS*.

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- Hatched strips: ... *Acacia woodii* savannah.  
 Enclosed Area: ... European maize farms.  
 ●: ... Trials on European farms.  
 ■: ... Trials in native areas.  
 □: ... Projected trials in native areas.  
 M: ... Mazabuka.



Introducing the above paper, MR. C. J. LEWIN expressed on behalf of the authors their thanks to the authorities at Kew for having identified various host plants referred to in the paper.

Cotton-growing in Northern Rhodesia, he said, was never likely to become very important as a source of the world's supply. At the same time, if the crop could be grown even in a small quantity by natives, it would be important to the Government of Northern Rhodesia, seeing that it would mean bringing money into the country from outside. At present the native population was entirely without a cash crop, with the possible exception of maize, and at the present time they were growing too much maize already. Thanks to the work of the Corporation's Station at Barberton, the jassid pest had ceased to be seriously troublesome, and the chief problems, other than that of the price of cotton, were bollworm and stainer. As regards the districts in which cotton had been grown by European farmers hitherto, he thought the position could be summed up by saying that *Dysdercus supersticiosus* took the first crop, *D. fasciatus* took the end of the crop, and the middle crop would probably be quite good were it not for the bollworm.

In the paper by Mr. Bebbington and Mr. Allan, the position with regard to stainer was described in some detail. He drew attention to the fact that there was still a missing link, as they did not know at present what happened to *D. supersticiosus* from the time that it disappeared completely in September until it suddenly reappeared everywhere in November.

Mr. Lewin also drew attention to the effect of the larvæ of the Cossid moth on the degree of incidence of the stainer population on cotton. If this moth developed in large numbers it destroyed the food (*Thespesia*) which the stainer preferred to cotton, and consequently drove it on to the latter crop. In other years, if the Cossid moth did not destroy the *Thespesia*, stainer for the most part remained on that plant and did not go on cotton. In the type of country in which cotton had hitherto been grown, it would always be a very speculative crop because there the perennial host plant of the cotton stainer, *Thespesia*, occurred in great abundance. The Government were consequently investigating the possibility of growing cotton by natives in areas free from that perennial host. Thanks to a very efficient ecological survey, various types of country had been mapped, and the districts that were free from this host plant were known. The results of the first year's work were distinctly encouraging. Although stainer had come into the cotton, it came in so late as to do no damage. He stressed the fact that only one year's work had at present been done, and it was impossible

to be sure what would happen after further seasons. He anticipated one criticism that he thought would probably be made, namely that what happened in an experimental plot probably would not serve in any way as a guide if cotton were grown on a large scale. He thought, however, that it was unlikely that cotton would ever be grown in the country on such a scale that there would be continuous fields of it, and they could only hope that if they were successful in avoiding stainer on the experimental plots, an approximately similar degree of success would attend the planting on the somewhat larger, but still not very extensive scale that was anticipated.

### DISCUSSION.

SIR GEOFFREY EVANS, *speaking on the paper by Mr. Parsons, said that in Queensland Heliothis was one of the main cotton pests, apart from occasional outbreaks of sucking bugs, e.g., Tectocoris. The work on Heliothis was started by Mr. Ballard, at present Entomologist to the Government of Palestine, and had been continued by the Queensland Government after he left. Sir Geoffrey thought that an account of the pest might interest the Conference.*

*The cotton belt in Queensland normally had a long dry season, from about the end of June until about the beginning of September. That was the cold season, when there was no growth of any sort. Usually towards the end of September there were light rains, 1½ in. to 2½ ins. coming in the form of thunderstorms. Those farmers who ploughed their land in the cold season, or at the end of the previous wet season, and had prepared a seed bed and could take advantage of those rains to do their planting, got an early crop of cotton which naturally grew very slowly until the major rains came towards the end of November or in December, and formed a short, stocky plant and produced invariably a heavy crop of cotton. Many of the farmers, however, at the time he was there, had a somewhat rudimentary knowledge of agriculture—many of them being settlers from overseas, as a matter of fact—and did not prepare their lands until the heavy rains came at the end of November or December. At that time they planted maize and cotton; it was the general planting season, and most of the farms were based on dairying and cotton. The maize was grown partly for fodder and partly as a corn crop, and the cotton was a sort of side issue upon which they depended for their cash. What happened was that the early crop of cotton, which was a short, stocky plant of very tough growth, came through practically without any bollworm whatever. The late-sown crop, sown under those heavy rain conditions and in a hot atmosphere, became very luxuriant. It was not affected until the maize crop was off, but when the late crop came into boll, it was immediately attacked by Heliothis, and it suffered very badly in the first and second picking, and all they got was a top crop.*

*This bore out the conclusions they reached, which were exactly the same as those of Mr. Parsons, namely, that Heliothis will always go for a maize crop in preference to a cotton crop, and secondly, it showed that the first-grown tough cotton was not affected by the Heliothis. If the Heliothis could not get maize, it attacked the second cotton crop. Trap crops of maize were tried, but in practice they did not work, owing to the difficulty of getting the farmers all to agree as to the right time to cut the maize crop and cart it. He suggested that Mr. Parsons would probably be interested to get into touch with the Queensland Agricultural Department and hear what further developments they had had in their work, if he had not done so already.*

MR. PARSONS directed attention to one point in connection with the results shown in Mr. Bebbington's and Mr. Allan's paper. The authors indicated that the area in which European farming was concentrated was an area where there was a sequence of the wild hosts of the stainer, which provided for continuous maintenance of different species. Elsewhere in the South-West and throughout a large part of Barotseland, however, as discovered in the course of the ecological survey, the stainer hosts were not represented in large tracts of country. This showed the advantage of employing methods of ecological reconnaissance survey in a new country from the point of view of cotton-growing.

A good deal was known about the species *Dysdercus*, and much was beginning to be learnt about the particular plant associations in which the wild hosts of the stainer occur. Had such information been available before, attempts might have been made some years previously to grow cotton in areas known to be relatively remote from aggregations of stainer hosts or at least from a sequence of them. The information they possessed should be used to explore ahead in areas where cotton-growing was projected. It should be profitable to demarcate areas on ecological data.

In some parts of Zululand, a wild host plant of the red, or Sudan, bollworm existed in definite associations; colonies of the plant grew together, and they undoubtedly formed the particular breeding-ground of this bollworm. The moth of this bollworm was very heavy-bodied. It could not be said that there was evidence that its range of flight was short, but taking into consideration certain natural barriers, such as mountain ranges and so on, Mr. Parsons suggested that it might have been advantageous in the early days of cotton-growing in Africa to have concentrated on those places where information on plant association and insect association with them had been marked down previously to the experimental efforts.

SIR GEOFFREY EVANS asked Mr. Parsons if he had any information as to the distance that stainer could travel.

MR. PARSONS replied that at Barberton they had on occasions marked stainers. Having collected stainers and put them in a refrigerator to dampen their activity, they had marked them and then released them at various points in connection with certain trapping experiments. Marked stainers had been recovered at a distance of two and three miles away from the point of release, but he did not attach very much importance to those observations. He had observed an uninterrupted flight for several hundreds of yards, and they were powerful on the wing. Having regard to the migrations that occur in cotton, and the distance away from cotton of the wild host plants, Mr. Parsons thought they must be able to fly at least a matter of miles, although not in one flight perhaps. When there was a series of wild host breeding-grounds dotted throughout the country, there was no reason to suppose that stainers could not filter, as it were, across a very wide area indeed.

Mr. Parsons referred in this connection to the importance of investigating closely the baobab forests in the Transvaal. Some 200 or 300 miles north of Barberton there were very large areas where baobab was abundant. It was well known that it was a host plant of *D. fasciatus*, and there was a gap in their information on the annual cycle of breeding this species. They expected to find that it was linked up with the large baobab areas far to the north. The distribution of the wild hosts of the stainers had been mapped to a certain extent. The baobabs are to be found in thousands near the Limpopo River, but as one comes southwards, they are much less abundant. The Southerly limit is practically on the Oliphant River, where a few trees only are found. This, however, is only 50 to 60 miles away as the crow flies, and it is possible that there may be a movement southwards of *D. fasciatus* to the immediate neighbourhood of Barberton, especially as there are other hosts between, which can serve to bridge the distance.

MR. ROGER THOMAS emphasised the necessity for concentrating on the breeding of those types of cotton that were most resistant to the most prevalent diseases and pests. He believed that a longer stapled cotton would be grown in the extensive irrigated areas of the Punjab and Sind if a resistant type could be found.

## PROBLEMS CONNECTED WITH THE CONTROL OF THE PINK BOLLWORM (*PLATYEDRA GOSSYPIELLA*, SAUNDERS) IN THE SUDAN.

BY

H. W. BEDFORD (*Sudan*).

*Introduction.*—The only regions in which it can be said that the pink bollworm (*Platyedra gossypiella*, Saunders) has proved to be a



serious menace to the cotton crop in the Sudan since the present Cotton Regulations have been in force, have been those in which the storage in native houses of seed cotton and cotton seed takes place in connection with the local spinning and weaving industry.

In Berber Province, where the cultivation of cotton and the weaving industry take place more or less side by side, the pink bollworm has been, and still is, the most serious pest and has been largely, if not entirely, the cause of the greatly reduced yields in recent years in most areas. In Khartoum Province the situation is somewhat similar, and owing to the low prices for cotton of recent years and the serious damage by pink bollworm, the growing of cotton has been considerably reduced and is now confined to three farms only, of which one is the Government Research Farm at Shambat.

In the Gezira conditions differ somewhat from those in Berber and Khartoum Provinces and the pink bollworm menace is by no means so grave, but its periodic increase there, particularly along the east of the Gezira Scheme, where the cotton areas are in closer proximity to the native villages near the river, calls for the adoption of further measures for controlling this pest.

*Relation of the local weaving Industry to the Control of Pink Bollworm.*—Prior to the introduction into the Sudan of Egyptian and American types the only cotton grown was of poor quality, known locally as “beladi.” Its cultivation was widely practised both under irrigation and as a rain crop, but more particularly in Dongola, Berber, Khartoum and the Blue Nile Provinces. The crop was grown entirely to meet the local demand for cotton and was used for spinning and weaving into the native cloth or “damour” and sundry other purposes. When American cotton was introduced into the above-mentioned Provinces, it largely took the place of “beladi” but the cultivation of the latter was continued as it was considered by the natives to be a better drought-resistant cotton and the lint more suitable for spinning and weaving. The latter prejudice was soon overcome and the employment of American and even Egyptian cottons for making “damour” steadily grew.

The growing of “beladi” cotton in the prescribed cotton areas has of recent date been prohibited, for it was realised that so long as the practice continued, the regulations prohibiting the sowing of any but “sunned” cotton-seed could not be enforced. Needless to say the “beladi” cotton suffered severely from pink bollworm.

The main danger to the cotton crop of the “damour” industry is the storage in native houses of infested seed cotton and



cotton seed, as the latter are considered to be the chief means by which the pink bollworm can carry over from one cotton season to another.

*The present position of the "Damour" Industry.*—There is reason to believe that the trade depression and prevailing low prices for cotton have tended to benefit rather than otherwise the "damour" industry. Cotton for this purpose is as easily obtainable as ever and even in Khartoum Province where the cultivation of cotton by natives has ceased, the "damour" industry continues to thrive, seed cotton being imported for this purpose from the adjoining provinces.

Although the Government is taking steps to try and prevent the storage of seed cotton by natives, it is at the same time encouraging the "damour" industry, as it provides a means of livelihood to many. There is, in addition, a growing local demand for "damour" owing to its better lasting qualities when compared with cheap imported cloth.

*Measures to prevent the storage of seed cotton and cotton seed.*—According to our Cotton Regulations the storage of seed cotton and "unsunned" cotton seed after a certain date is prohibited, and for each province concerned dates are fixed annually when the regulation comes into force. The application of such a measure in areas in which the "damour" industry flourishes is a difficult problem.

In Berber Province various efforts have been made in the past to prevent the storage of seed cotton and cotton seed by propaganda and by offers to purchase any which is brought in, but with little or no effect. At the end of last season a cotton "clean-up" was carried out in the Shendi District by the Administrative Staff in co-operation with the Department of Agriculture and the Entomological Section, which included an inspection of native houses within the area. This resulted in quantities of seed cotton and cotton seed being collected, but its effect on the pink bollworm infestation the following season was disappointing. The apparent failure was probably in large part due to the movement of seed cotton into the Shendi area from outside after the clean-up had taken place.

One distinctly beneficial effect of the clean-up was that it created a demand for cotton where none was obtainable except through the Agricultural Department. This demand was met by the issue of ginned cotton, against the use of which for spinning purposes there had previously been a strong prejudice. The experiment was a success, and since then a certain amount of ginned cotton was issued free of charge to the various weaving centres as a form of propaganda. In addition, its sale at a very low price to sheikhs for distribution where required has been authorised by the Director of Agriculture, and it is hoped that the demand for it will grow.

In preparation for the coming season the natives have been warned to have all seed cotton in their possession ginned and the seed destroyed by a certain date, after which a further clean-up will be carried out and, if possible, will include all the cotton areas in the Province. Propaganda will be continued to encourage further the use of ginned cotton for the "damour" industry and other purposes.

In the Gezira an attempt was made in 1934 to force all natives holding seed cotton to have it ginned by a fixed date, after which all seed brought in to various centres was purchased. As a result 15,634 rotlis of seed was obtained which was estimated to contain 62,536 living pink bollworm. Similar measures including inspections of villages have been carried out at intervals subsequently with variable results. As the Gezira Scheme expands so are the difficulties increased of putting into effect measures of this nature. What is required is some more lasting means of preventing the demand for seed cotton, and the only way of achieving this end would seem to be either the prohibition of the "damour" industry within the Scheme or the adoption by the natives of ginned cotton for this purpose.

In the absence of Mr. Bedford, MR. W. P. L. CAMERON opened the discussion by giving an outline of the attempts which had been made to control the pink bollworm, now present in all the larger cotton-growing areas of the Sudan.

The great potential destructiveness of that insect as a cotton pest was well known, he said, and its control was of fundamental importance in that it imposed restrictions on the growing and disposal of the crop which were at times extremely irksome to all concerned.

In the Sudan, no practical measure had so far been found for controlling the pink bollworm during the growing season when damage was in progress. Efforts had been made to obtain biological control by means of *Microbracon kirkpatricki*—the only parasite worthy of consideration—but the small success obtained had not seemed to justify the labour and expense involved.

In practice, therefore, the only hope of control lay in preventive measures, the basic features of which were the very comprehensive cotton regulations of the country. Briefly, those regulations were designed to obtain control over the seed, the growing and disposal of the crop, and the establishment of a "dead season"—not shorter than two months—between the crop of one season and that of the next when no

host plants for the boll-worm would be available. In the Sudan, the "dead season" coincided with a period when extremely high temperatures, drought and consequent low humidity prevailed.

The difficulties involved in insuring that those regulations, which were of prime importance in the work of pest control, were adhered to, were greater in some districts than in others. The greatest difficulty had been experienced in Dongola, Berber, and Khartoum Provinces, where, after the bulk of the crop had been collected and sent to the factories for ginning and ultimate export, the natives retained a proportion of the crop for spinning and weaving and general domestic purposes. A large proportion of that seed cotton constituted the "tail" pickings of the crop, and in consequence contained a high percentage of pink bollworm. It was that native practice which was considered to be the chief cause of the annual pink bollworm infestations.

The importance of getting hold of that seed and the difficulties in preventing its storage by the natives during the "dead season" Mr. Bedford had amply stressed. He had also described another attempt to meet the situation which, if successful, would be to the benefit of spinners and growers alike.

In the case of the Blue Nile Province where the spinning industry was, Mr. Cameron thought, not so firmly established or at any rate not so general, there was much more at stake than in the other Nilotic areas. An enormous area of cotton, the Gezira, had to be considered, in which not only were more natives interested financially, but also the Sudan Plantations Syndicate, and particularly the Government, and the idea of allowing the spinning industry to exist in close proximity to such a scheme seemed to involve considerable risk. That had been the view of the Governor of the Province in 1928 when, following a season of high pink bollworm infestation and after much propaganda and due warning had been given, a thorough clean-up and house-to-house search was undertaken during the summer months. That resulted in heavy fines in many cases, and was an unpleasant and arduous task for the Administration, whose help was essential, but he thought it could be claimed to have been a success, judging by the marked decline in the bollworm figures of subsequent years. In undertaking measures of that sort, he considered the essential feature was that they should be carried out annually and extensively—possibly with the issue of ginned cotton at a cheap rate to alleviate any hardship—so that much of their unpleasantness might disappear, and the native would come to regard the clean-up at the end of the cotton season merely as part of his routine duties.

Once the legislative side of the control work was placed on a firm footing, it would then be possible to consider the relative importance of minor fluctuations and sources of infestation in the larger cotton areas. Some of those the Entomological Section had already investigated and continued to investigate.

Of those, the first was the significance of alternate host plants. So far, *Hibiscus esculentus* had been the only alternate host plant on which the pink bollworm had been found, and that only on rare occasions.

Then, the possibility of the pink bollworm being able to survive the "dead season" in a resting stage in the soil in varying circumstances still received attention. A good deal of work on that subject had been done in the past by three Entomologists at different times, but with negative results.

Another possible source of infestation was the seed used for sowing. That, however, was not considered likely, as no living pink bollworm had ever been found in samples of seed which had been sun-treated under supervision. In the southern region of the Sudan, however, a heating machine had been installed, and it was not known yet to what extent it was reliable. The views of others regarding the efficiency of such machines would be most welcome.

Furthermore, a study of the bionomics of the resting stage of the insect was of great importance. In the Sudan, the period at which the pink bollworm started to produce long cycle or resting larvæ varied according to the date of sowing and the age of the crop. The resting stage was not regarded as a state of hibernation or æstivation, but rather as a state of rest induced by impending unfavourable conditions. The factors directly responsible for bringing about that change were not considered to be definitely either climatic or due to lack of food, but were believed to be influenced by chemical changes in the composition of the food which took place as the plant reached a state of maturity. The earliness or lateness at which the larvæ entered the resting stage did not seem to bear any relationship to the period of emergence of the moths. In the Northern Sudan it had been found that moths emerged from resting stage larvæ, chiefly during the period mid-July to the end of September, which corresponded to the rainy season. A few moths might emerge at wide intervals later.

There were one or two important points which still required investigation. Any views on the length of life of the moths which emerged from long cycle larvæ when no host plants were available on which they could oviposit would be extremely interesting. In



addition, information on the extent of flight of the pink bollworm moth would be most welcome.

In conclusion, Mr. Cameron considered that a comparative study of the pink bollworm throughout the season should be undertaken to see what differences existed, if any, in the behaviour of the insect in the larger cotton-growing areas of the country. Before this could be attempted, however, he maintained that it would be essential to make quite certain that the same legislative measures were being strictly carried out in all parts.

#### DISCUSSION.

MR. SAMPSON *suggested that one possible method of control might lie in the choice of the variety of cotton grown in any particular district. When he first went to Madras, Pink Bollworm was of no significance as a pest on the Asiatic cottons that were grown there; when Cambodia was introduced, however, Pink Bollworm became of considerable importance. He did not know how far it might be possible to breed a resistant Asiatic cotton that might compete with these New World cottons which seemed very susceptible to this pest, nor did he know whether their root systems would render them adaptable to irrigation conditions.*

MR. JACKSON *said in reply that they had found at Indore that Asiatic cottons gave phenomenally high yields under irrigation, so that there did not seem to be any reason why their root system should not be tolerant of such conditions.*

MR. ALLEN *mentioned that in the United Provinces they grew nothing but Asiatic cottons, and the incidence of Pink Bollworm was extremely heavy.*

MR. SAMPSON *suggested that the extent to which Pink Bollworm attacked an Asiatic cotton might be due to the size of the boll. The boll of Madras indigenous cotton was only small, whereas that of United Provinces cotton was very much larger. He merely suggested this as a possible explanation.*

DR. WILLIAMS *said that it appeared from Mr. Bedford's paper that the conditions in the Sudan at the present day closely resembled those which existed in Egypt about ten years ago; there were, however, two essential differences, one to the advantage of the Sudan and one to the advantage of Egypt. In Egypt they had not the complication of the native spinning industry, but, on the other hand, they had the very big complication of the impossibility of burning the cotton sticks in the fields, which, in the complete absence of any other fuel, had to be stored for fuel purposes; and it was believed to be one of the major sources of infection of the cotton crop.*

*With regard to the length of flight, he did not know of any large-scale experiments, but he remembered that in Egypt they had*



found Pink Bollworms in a light trap in the desert about 12 to 15 miles away from the nearest cotton. That was merely an isolated experience, but it did throw some light on the subject.

DR. BALLS said some information had been asked for with reference to heat treatment of seed. All seed used for sowing in Egypt was heat-treated in an installation in the ginnery. Before any sack of seed was passed for sowing, samples had to be drawn and sent up to the Ministry of Agriculture, where they went through a double examination, one a test for the amount of non-typical seed in the sample ; the other for a germination test, to see that it had not been injured by the heat treatment, and to get a record of the number of pink bollworms, dead or alive—there were practically none alive—found in the seed. A vast mass of figures was being accumulated which represented the whole crop in Egypt for many years past ; they were in process of being worked out.

The man who was handling pink bollworm in succession to Dr. C. B. Williams was Ibrahim Bishara Effendi, who was obtaining very interesting results as to the infestation of seed with pink bollworm from cotton sticks. Bishara at first used a survey area of a square mile or so ; this he increased gradually to districts between two and three villages, and during the previous year he had got a gigantic experimental area covering fifty kilometres square, in which, by special arrangements, all carry-over cotton sticks were being abolished, while in the adjacent fifty kilometres things were being left as they were. The control observations were taken in the previous year, and the present year would provide the direct experimental demonstration. As Dr. Williams had said, in Egypt they had to put up with the cotton sticks because they were an important fuel. One might say "Strip the bolls off the sticks and destroy them." If the figures were worked out, it would be found that the weight of bolls to be handled was about ten times the weight of the cotton crop of Egypt. To make any use of those bolls for paper-making, or anything like that, was out of the question ; their value was too negligible ; but Bishara expected to show that the amount of damage emanating from those bolls would even justify the Government in buying them up, merely to destroy them in the interests of the cotton crop at large. But the essential feature of his results was that the rate at which the infestation fell off as one went outwards from any place where the sticks were stored was very rapid. Dr. Balls explained that he was speaking rather of the early part of the season, the green boll infestation : the change with distance flattened out later. The easiest way to demonstrate the fall-off was to take the halving change—that is to say, immediately round a village there were a lot of cotton sticks on the roofs of the houses, and a certain percentage of bollworm was found in the cotton crop. Less than 200 metres away from the first observation point the infestation was halved. Another 200 metres away and it was halved again : and that held good up to at least a kilometre and a half, by which time

one was probably entering the sphere of another village or another storage place. This had been confirmed in five successive years, and always the order of falling-off of the green boll infestation was a halving change of the order of 200 metres—a most remarkable result. He added that those conclusions did not depend on having just a few samples along a line. In the big area that Bishara was handling, he had got sampling points, he believed, at every 100 metres over the whole area. Certainly the infestation fell off with a very cheerful rapidity, and if those storage points could be eliminated by any means, there would be a great reduction in the average infestation of Egypt.

DR. NEAVE, in summing up the discussions on the entomological papers, pointed out that the full history of nearly every insect pest seemed to be very inadequately known, and he thought that the Entomologist must in future undertake a much more intensive and accurate study of the reaction of insect pests to their ecological environment.

## COTTON DISEASES.

Chairman: DR. E. J. BUTLER, C.M.G., C.I.E., F.R.S.

The following paper, and that on Blackarm in Uganda which succeeded it, were discussed together.

### ANGULAR LEAF SPOT AND BLACKARM OF COTTON CAUSED BY *B. MALVACEARUM* E.F.S.

BY

R. E. MASSEY (*Sudan*).

*Causal Organism*.—A motile, non-sporing, slime-forming bacterium. Highly resistant to desiccation and sunlight, especially in dried slime.\* Within the cotton plant it invades the parenchyma, rarely penetrating the bundles, and is never truly vascular. Progress is intercellular by slime threads, but cells are digested and cavities formed. Boll infection is a most serious aspect of the disease in the Sudan, as the cavity of the boll is entered usually via the base, and seed infected by the intrusion of the slime.

*Alternate Hosts*.—None found in the Sudan. It is remarkable that healthy leaves of an infected cotton plot may during the rains

\* A series of organisms morphologically resembling *B. malvacearum* E.F.S. has been isolated from cotton, but none except the typical smooth form of *B. malvacearum* is pathogenic.

carry millions of viable organisms, and yet *B. malvacearum* has never been isolated from the surface of weeds growing within the plot.

*Sources of Infection.*—(a) Infected seed, (b) Fallen infected bolls, (c) Infected plant remains, particularly woody tissues, (d) Soil.

(a) Seed is mainly infected externally, the fuzz hairs holding the organism in a viable condition for as long as two years. Seed is often contaminated in the ginneries.

(b) Fallen infected seed germinating with rain is a fruitful source of trouble. Under suitable conditions of soil, moisture and temperature seedlings bearing characteristic lesions result, and the disease is thereby spread to neighbouring cotton by rainstorms.

(c) The survival of the organism in plant remains is a serious aspect of the disease and one difficult to control. It is impossible to avoid contamination of new areas unless the most rigid precautions are taken. In the dry state diseased stems and bolls will retain the organism in a viable condition for a longer period than usually obtains between crops. Moreover, partial wetting with subsequent drying only slowly effects sterilisation. Complete flooding of debris for four days has been proved to render the most heavily infected plant debris innocuous.

(d) Luckily the organism has no separate life in the soil, and has never been isolated apart from plant debris one month after inoculation. Fine road dust similarly does not hold infection to any serious degree after two months' exposure in the open, even when debris is mixed therein, for comminution appears to hasten the death of the contained parasite.

*Transmission.*—Wherever the disease is endemic a heavy driving rainstorm may liberate the organism from fallen debris lying on the fallows and carry it to healthy cotton nearby. The parasite can only enter the host plant in the presence of moisture and when the stomata are open. The application of water to a fresh lesion results in the almost instantaneous liberation of the organism, and entry of open stomata is a matter of moments. Motility is not necessary, as water appears to be drawn into the stomatal cavities. The organism may lie on the surfaces of healthy leaves in a viable condition for many days and enter with the aid of dew; also, under humid conditions, infection may result by contact. The distance to which the parasite may be carried by rainstorms is not known. It has been tracked 250 metres, but by far the worst

damage occurs at close range, and within a crop is confined to a small area.

*Incubation Period.*—Visible lesions may be formed in young tissues within four days of the entry of the parasite, but the incubation period is greatly affected by atmospheric temperature and humidity, and also by the age of the tissues. Senility confers a high degree of immunity.

During the rains lesions appear on the cotyledons of germinating seed some 8 to 10 days after sowing, but at a later period signs of disease may not appear on seedlings until three weeks have elapsed.

*Conditions necessary for Infection.*—(a) Continuous high humidity is not vital, but exposure to high humidities results in a greater degree of infection, as more bacteria enter before the surface of the leaf dries. Dry spread is unknown. Humidity is more important than temperature, but, provided that the relative humidity is high, a higher air temperature results in greater infection, when plants are sprayed.

(b) During the germination of exteriorly infected seed, free water is necessary for the migration of the parasite from the seed coat to the cotyledons within the opening seed. There is also a time limit for infection, as in the Gezira *B. malvacearum* dies in wet soil within 48—72 hours. A bacteriophage has been isolated from the soil of plots that have borne a diseased crop; also Gezira soil exerts a strong dissociating action on cultures of *B. malvacearum*, leading to the formation of non-pathogenic R. form, and possibly, other transitional stages of the organism.

It will, therefore, be clear that the amount of infection resulting from infected seed is greatly influenced by soil moisture and temperature. A healthy crop can be raised in a well-drained soil, with a temperature outside the range at which *B. malvacearum* operates. Optimum conditions for infection are a wet soil (45 parts of water per 100 dry soil in the Gezira), and a soil temperature round about 25° C. In a field a rainstorm will cool the soil from 30° C. to 20°—25° C., adding at the same time the necessary moisture.

*Measures for Control.*—(a) Clean seed—disinfection by dusting or steeping is advocated. Delinting with concentrated sulphuric acid may not be effective.

(b) Feeding off the old crop to cattle. It has been ascertained that *B. malvacearum* does not survive in the alimentary canal of a goat, nor in weak hydrochloric acid (2 per cent.).

(c) The collection and burning of infected plant remains, particularly woody tissues.

(d) Where possible the flooding of infected lands for four days.

(e) Where (d) is not possible, rotation with a rapidly growing smother crop might limit the dissemination of infected debris. Protective screens to the new crop are worth trial.

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MR. MASSEY said that the Summary supplied to the Conference covered the salient points connected with the control of the disease. Further details were contained in an article printed in the July number of the Empire Cotton Growing Review. It only remained, therefore, to emphasise the importance of crop hygiene, *i.e.*, the disinfection of all seed used for sowing and the clearance of all infected particles from the field at the end of the crop. Both measures were being tested in the Gezira in the present year, where some 2,300 feddans (acres) were being flooded for four days, after clearance of all material that could be either grazed to cattle and goats or actually brushed up by hand. Debris had been tested before and after the completion of the operations with most promising results, all types of debris being found to be entirely disinfected wherever flooding had been thorough.

## BLACKARM DISEASE IN UGANDA.

BY

C. G. HANSFORD (*Uganda*).

The following pages contain a short summary of the work carried out in Uganda since 1931 on this important disease of cotton. It is assumed that all members of the Conference are familiar with the symptoms produced by the disease. Its history in Uganda is as follows :

In 1922-23 there is a note in the records of Serere Experiment Station to the effect that in that season all the cotton grown on the Farm was attacked by an unidentified disease, reducing the yield to about 140 lbs. of seed cotton per acre. From the description there given there is very little doubt that this was Blackarm. There is then a gap in our information till 1925-26, when Snowden definitely identified the disease as occurring in Uganda, and in that year he described the leaf form as of common occurrence.



## BLACKARM DISEASE IN UGANDA.

In the following season, 1926-27, Angular Leaf Spot was of almost universal occurrence in the Eastern Province, and extremely common west of the Nile. The stem form occurred in quantity on Serere Farm and was accentuated by a severe hail storm.

In 1927-28 the stem form of the disease was less common in all areas, though the leaf form was practically universal.

In 1928-29 the northern and eastern parts of Teso district suffered very severely from the stem form of the disease; a very poor crop was reaped in these areas. The year 1929-30 is still referred to all over the Eastern Province as "the Blackarm Year." The loss of crop attributed to the effects of the disease is estimated at some 50,000 to 70,000 bales. The heaviest losses occurred in Teso, Lango and Bugwere; the average yield over Teso district in that season was only 51 lbs. per acre, as compared with the average over the whole period, 1920-32 of 237 lbs. per acre.

For the following crop the seed supply was taken from the least severely attacked areas in Busoga; and less damage was reported. The crop showed severe infection by the leaf form of the disease early in the season, but the stem disease did not appear in epidemic form as in the previous season. Teso district during 1930-31 produced its record crop to date.

1931-32 was on the whole a season of low cotton yields, but these were rarely due to attack by disease. The heaviest losses were undoubtedly due to excessive flower and boll shedding caused by the unusually prolonged periods of moist weather.

In 1932-33 and 1933-34 seasons damage caused by Blackarm was limited to small areas of the Eastern Province.

*Origin of the Stem Form of the Disease.*—During the past few years all information collected in Uganda points to the conclusion that the stem form of the disease arises in some 99 per cent. of cases from an extension of the infection on leaves and on the bracts of the bolls. On the leaves two types of infection occur:—(a) in which the lesions are scattered over the whole surface of the leaves, and (b) in which the lesions are mainly confined to the immediate vicinity of the large veins of the leaf, often coalescing to form long lines.

It is from the latter type of leaf infection that the great majority of stem lesions arise. Under favourable climatic conditions the vein lesions on the leaves extend downward through the softer tissues of the petioles into the stem. The progress of this extension from day to day can be noted with the naked eye, as the infected tissues are rather different in appearance from those still remaining healthy.

Simultaneously with the extension of the petiole lesions the supply of water and food to the leaf blade is interrupted to an increasing extent, so that the blade gradually withers and dies. As a result of such withering the abscission layer is formed at the junction of petiole and stem. Through this abscission layer the bacteria appear to be unable to penetrate, so that the general process of invasion of the stem tissues via the petiole may be pictured with reasonable accuracy as a race between the progress of the bacterial invasion of the tissues of the petiole and the formation of the abscission layer. Any condition of climate or soil which tends to hasten the formation of the abscission layer will help to reduce the incidence of stem lesions. Among such conditions a few days of dry hot weather are probably the most important, as then the drying out of the leaf blade will be accelerated, as well as that of the petiole itself, which latter will help to retard the rate of bacterial invasion.

In passing, it is worthy of note that an index of what may be termed "potential" stem infections can be obtained quite late in the season by observation of the number of dry leaves still hanging from the stem and branches of the plant. For practical purposes it can be assumed with safety that all these are leaves in which infection has spread from the leaf veins through the petiole tissues. Actual infection of the stem may or may not have occurred, but it is characteristic of this infection process that such leaves tend to remain attached to the plant long after normal senile leaves have been shed.

*Spread of the Disease.*—The process of the development of the stem form of the disease has just been described; it follows from this that the stem form does not spread from plant to plant as such, but rather that an epidemic of the stem form depends on special circumstances, which, fortunately for Uganda, appear to occur comparatively rarely over large areas in any one season. First, the climatic conditions must be such as to ensure a heavy leaf infection on the plants; secondly, conditions must favour the spread of the bacteria through the petioles; thirdly, the leaf blade must not dry out rapidly, otherwise the abscission layer will be formed before invasion of the stem takes place. Most of these conditions obtain during long spells of uninterrupted moist cool weather, but two or three days of hot sunny weather appear to act as sufficient control of the process.

It will be inferred, therefore, that spread of Blackarm, *i.e.* stem disease, may or may not follow on spread of the leaf form (Angular Leaf Spot). The latter is spread by two main agencies: (a) wind-blown rain, and (b) surface water. The directional spread of the disease is a resultant of the direction of these two agencies, of which the second appears to be the more important under Uganda conditions. During moist weather

the bacteria in the leaf lesions emerge on the surface in the motile state in the film of water on the leaf, and light rain storms driven by wind may transport the bacteria by splashing to neighbouring plants. If, however, as is often the case in Uganda, the wind-blown storms are heavy, the bacteria get washed off the plants into the surface soil water and infection of surrounding plants may not take place. Should the whole of the surface water be absorbed by the soil, spread of the disease may actually be retarded by such storms. On the other hand, during heavy storms, the surface water usually drains off the land, and may run for considerable distances through the cotton fields. Any splashing of this contaminated water by the tail end of the storm on to uninfected plants will leave the bacteria on these under ideal conditions for infection through the stomata of the leaves, stipules and bracts.

No reliable evidence has yet been collected in Uganda of any relation between the incidence of insect pests and the spread of this disease.

*Pathology of Stem and Branch Infections.*—Once entry of the bacteria into the cortical tissues of the plants has been accomplished, the lesion caused by them extends in a downward direction as long as climatic conditions are favourable, *i.e.* as long as cool moist weather continues. On the advent of hot dry weather, the extension of the lesions is retarded, and finally ceases. When the lesions cease to extend, the plant forms a corky layer, cutting them off from uninfected tissues, and if conditions remain hot and dry this layer is completed and acts as an efficient barrier to further extension. The infected tissues dry out, and are finally removed as corky bark, rather different in colour and surface texture from normal bark of the plant. Such dry lesions can be seen even at the close of the season when the crop is finished and the plants pulled up.

*Effect on the Crop.*—The sum of the effect of the disease on the crop depends on several factors :—(a) the number of lesions per plant ; (b) the time of infection in relation to the development of the plant ; (c) the site of the infection ; and (d) extent of individual lesions.

Taking these in turn, the number of lesions per plant obviously will have an effect on the crop produced. In this connection it may be noted that on cotton varieties commonly grown in Uganda, we have little or no indication that the leaf form of the disease as such has any effect at all on crop production, except that without leaf infection being present, infection of the stem will not occur.

The two varieties of cotton at present grown in the Eastern Province, where Blackarm is of more economic importance than in Buganda,

are N.17 and S.G.29, both derived from Nyasaland Upland American. On these varieties there appear to be two periods when stem infection is of great importance in its effect on yield. Of these the first occurs during the early development of the plant, at the time when the three or four basal monopodia are developing, up to the time they reach a length of 6 to 8 inches. If serious infection of the main stem or of these monopodia occur at this stage, the whole plant may be checked in its development, and may even remain stunted all through the season, producing little or no crop. Serious infection at this period often results in the breaking of the main axis of the plant, and death of the portion beyond the infection. Many plants may die out completely. The young monopodia may be affected in like manner, and even be completely suppressed. The effect of this complete suppression of the basal monopodia on the variety S.G. 29 is to reduce the crop to one-third of that of normal plants, even in the absence of damage to the main axis. On the other hand, if such plants are situated in good soil and given favourable climatic conditions, they may be enabled to compensate for the suppression of the early monopodia, by developing others higher up the main stem, in which case the crop may be increased to some 80 per cent. of the normal. Reduction of the basal monopodia to one, instead of the usual three or four, results in a crop production of some 60 to 75 per cent. of the normal. Thus we deduce that early serious infection on the monopodia may reduce the potential crop by from one-sixth to two-thirds.

The second period during which heavy infection may have a serious effect on the crops is during the months of September and October. During this period the sympodia at levels of one to three feet from the ground are developing and producing flower buds. Infection of these sympodia may cause total loss of the potential crop borne on them, the loss being proportional to the number of sympodia attacked. During the "Blackarm Year" of 1929-30 the plants which had more or less recovered from the early infection period showed a complete gap in the levels corresponding to the second period under consideration. All these sympodia were completely dead. This part of the plant is of major importance in the production of the "first" crop under Uganda conditions, especially so in view of the fact that it is only in what may be termed exceptional years that the so-called "top" crop can be expected to yield an important addition to the cotton crop of the country.

For the destruction of the "first" crop produced by the sympodia of the middle part of the plant, it is not essential that large lesions be formed on the sympodia themselves; all that is required is infection of the first node and of the terminal region of each sympodium. If



this occurs, no further growth can take place, and the sympodium quickly dries out to a stick. It is likely that infection by way of the stipules plays a not unimportant part in infection of the terminal part of the sympodium. Such infection is known to occur here, but detailed observation on this point is very difficult, and attempts at estimation of its importance have given very doubtful results.

Another method of infection of the sympodia frequently occurs. The first leaves produced on them may be shed owing to petiole infection or from other causes, and no stem infection take place. Then the plant develops a set of small secondary leaves on these parts, which appear to present less difficulty to invasion of the tissues by the bacteria. Through these the bacteria gain entrance to the stem tissues, causing the drying up of the sympodia. This process often takes place later in the season, after the main foliage has been reduced during a hot, dry period.

Although initial infection of the sympodia may take place at the period mentioned, a dry spell immediately succeeding may control the development of the stem or branch lesions, and cause them to dry out completely, the result being less damage to the final crop. This actually occurred during the 1933-34 crop.

With reference to the effect of the site of the lesions on the plant, this has been discussed above in dealing with the time of infection. There is another aspect of this, however, namely that in some years there is a considerable amount of infection spreading through the tissues of the bracts of the bolls, into the stalk and base of the boll, and even into the branch below. This type of infection, when occurring on young bolls, usually causes them to shed, while older bolls either dry up completely before reaching maturity or often open prematurely, producing no marketable cotton. Or, again, should they be able to continue their development, the bacteria may invade any or all the contents and tissues of the boll, resulting in a type of disease which could be included under the term "Internal Boll Rot."

*Carry-over from Season to Season.*—We have no evidence that in Uganda there are alternative hosts of this disease. In the Sudan great importance has been attached to the presence of volunteer cotton in neighbouring plots, carrying the disease in active form. Under the special conditions of native agriculture in Uganda, where, in the Eastern Province, the cotton plots are sown with the cereal *Eleusine coracana*, often before the cotton is uprooted and burnt, volunteer plants are not of major importance, as a source of infection. In weeding the food plots such plants would be destroyed. Also much of the cotton is grown on newly cleared land, so that the possible carry-over



of disease in the soil becomes of less importance. A small trial laid down to determine whether the bacterium could remain in the soil in rubbish from a previous infected crop yielded no evidence of this.

Under Uganda conditions all the available evidence points to the conclusion that the only important means of carry-over of the disease is on or in the seed. A large number of experiments and observations have been made on this point, and the results of these indicate that while internal infection of the seed is known to occur here, it is almost infinitesimal. Counts made indicate that such infection occurs on the average in about one seed per 10,000. On the other hand, infection of the fuzz on the outside of the seed is very common, and in some samples of seed may amount to as much as 10 per cent., as indicated by counts of cotyledon infection on the seedlings. Such being the case, investigations have been made during the last three seasons into the effect of seed treatments in reducing the amount of infection on the seed. Sulphuric acid has been recommended in the U.S.A., and gives fair results in Uganda. Unfortunately its use is entirely ruled out on account of the expense of treatment of large quantities of seed, and of the danger entailed by its use by more or less naked natives under very inadequate supervision. Our seed supplies are never concentrated at a few centres; in fact, seed distribution regularly takes place at nearly 200 ginneries, most of which are run by Indians.

Dusting the seed has given quite good results in experiments, but here, again, there are immense practical difficulties in applying this on a large scale in Uganda. The native cultivators have been accustomed to obtain the majority of their seed requirements early in the planting season, so that without further delay they can plant it as and when suitable weather is experienced. This system works well, and there is no reason for its discontinuance. The seed is often stored in the cultivators' huts or food stores, where it comes into close contact with food and cooking utensils. As the compounds used for dusting seed are intensely poisonous, the danger of indiscriminate distribution of seed thus treated is obvious.

The practical difficulties in the application of large-scale seed treatment in Uganda have indicated that another approach to the problem is necessary. This is discussed next under the disease resistance of various cotton varieties at present available here.

*Varietal Resistance to Blackarm Disease.*—Since the "Blackarm Year," 1929-30, increasing attention has been devoted to the selection of varieties more resistant to the disease than N. 17 and S.G. 29, the present standard varieties. It may be stated at once that we have no variety of cotton in Uganda completely immune from it.

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It was noted at the outset that certain plants in some U. 4/4/2 cotton grown at Serere, received from Parnell in South Africa, showed less damage from disease than our standard varieties. Unfortunately the lint of this variety was totally inferior to that required by Uganda standards. Efforts have been made to isolate a variety which will combine the high yielding capacity, drought resistance, Blackarm resistance and jassid resistance of this type of cotton with the lint characters required for Uganda cottons.

The most recent information available indicates that a great deal of progress has been made in the right direction, and certain of the latest selections show a fairly close approach in lint characters to our standard S.G. 29. There seems every hope of our being successful in this matter in the near future.

A second family of Blackarm-resistant or tolerant strains of cotton has been derived from a single plant discovered by Mr. Hosking in a native plot. Unfortunately in this case there is a considerably amount of jassid susceptibility.

A third source of varieties more resistant to Blackarm than our present cottons is the series of hybrid U.4 cottons received last year from Dr. Harland in Trinidad. Some of these appear to contain distinctly promising material for selection.

Finally, efforts are to be made this coming season to obtain hybrids from U. 4/4/2 derivatives and our best spinning cottons S.G. 23.8 and S.G. 29.

*Conclusion.*—I have omitted in this paper to discuss the effect of soil conditions upon the disease in Uganda, for the reason that we are able to do very little to remedy poor soils. Some of our areas where cotton is grown at present could be classed as definitely not suited to cotton, but we have no alternative crop for such areas in view of the present depressed state of the world's markets.

In conclusion I have to thank the Botanical section of the Uganda Department of Agriculture for allowing me to incorporate here a very brief résumé of the progress made in selection of disease-resistant cottons. I have endeavoured to assist in this work as much as possible, as on it is likely to depend the whole future of Uganda as a cotton-producing country. I understand that Mr. Hosking will be present at the Conference, and he is better able than I am to give information on this subject.

Mr. Hosking said that, not being a mycologist, he felt some trepidation in introducing this paper in Mr. Hansford's absence. There was very little he could add to the paper, but there were some remarks he would like to make on one or two points to which Mr. Hansford had already drawn attention.

There was a reference in the paper to what was known in Uganda as the bad Blackarm Year. In North Teso, which was the most severely affected part of the country, the average yield per acre was less than 20 lbs. That year Blackarm was associated with jassid.

On later pages, there were references to a certain type of lesion in which the lesions were mainly confined to the immediate vicinity of the large veins of the leaf, often coalescing to form long lines. It might be mentioned that those lesions were sometimes associated with aphid.

Referring to the paragraph about the two varieties of cotton, N. 17 and S.G. 29, Mr. Hosking said that both of those cottons were Nyasaland types. The Nyasaland type of cotton usually had three or four large monopodia. The yield was not good; the plant was rather straggling and tall, and care was needed in inter-cultivation not to damage the monopodia; an experiment which was done by Mr. Hansford showed that the suppression of the large basal monopodia by Blackarm resulted in an enormous reduction of crop. So far they had failed to select non-monopodic forms from Nyasaland Upland cotton. It was one of the great points in favour of the derivatives from U. 4 and U. 4/4/2, the original seed of which was very kindly supplied by Mr. Parnell, that U. 4 was not monopodic in type; and thus large reduction of yield could not result from the suppression of monopodia.

Another point about U. 4/4/2 was that it had been found possible to select strains which, though not resistant, were very tolerant, to the disease. The figures obtained last season were very striking in this respect. In the usual large variety trial, Mr. Hansford carried out frequent infection counts on all plants in the trial. He found in the early part of the season that the U. 4/4/2 derivatives had only 5 per cent. infection—that is, percentage of plants showing any form of Blackarm infection or angular leaf spot. At the same stage the Nyasaland Upland derivatives had 25 per cent. Later on in the season lesion counts were made, and the leaf spot counts were discontinued, because after a certain stage it was considered that angular leaf spot ceased to be important, and it was merely the lesions on the stems, monopodia and sympodia, which were worth counting. Speaking from memory, Mr. Hosking said the figures were about 25 per cent.

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for the U. 4/4/2 derivatives, and over 80 per cent. and up to 90 per cent. for the Nyasaland Upland derivatives. The comparative yields at the end of the season showed over 30 per cent. in favour of U. 4/4/2 derivatives.

The work that was being done by Mr. Hansford on the subject of Blackarm could roughly be divided into two sections: he was investigating the disease firstly from the mycologist's standpoint, from the point of view of trying to find some method of practical control of infection, such as dusting. Secondly, Mr. Hansford and he were following up a line from which they had far more hope of success, namely, the selection of strains or individual plants which showed a marked tolerance to the disease.

Mr. Hosking referred to a plant which he found in the Blackarm Year, as mentioned in Mr. Hansford's paper. That plant was growing in the most heavily infected part of Uganda, which was North Teso. It was obviously hybrid, because of its size: it had had lesions on it, but it had a few open bolls. Seed was collected and planted out the next season, when the progeny split for many characters. He had continued with this for about four years, and it was beginning to be fixed for lint characters and for habit, but unfortunately it was susceptible to jassid. Jassid did not occur in Uganda every year, but it was necessary to watch it very carefully, so he was afraid he had not much hope for that particular cotton.

Mr. Hansford mentioned in the paper that they wished to obtain hybrids between the U. 4/4/2 derivatives and their best spinning cottons, S.G. 23.8 and S.G. 29. S.G. 23.8 had the best spinning characters of any cotton that they had produced to date, but it had one very bad disadvantage as far as the Eastern Province was concerned: it was jassid-susceptible. S.G. 29 was fairly susceptible to jassid and very susceptible to Blackarm, but it might be possible to fix the good lint character on to the U. 4/4/2 type of plant, which had not yet given the type of lint that they required. Selection in U. 4/4/2 had, however, resulted in a 4 mm. increase of staple length since it had been in Uganda.

There was a reference in the paper to the effect of digging in infected plant material into so-called virgin soil. In a replicated, randomised experiment last year large masses of old cotton material were dug in to a depth of at least three inches in certain plots. Another series was planted with sterilised seed, the seed being dusted with one of the German dusts, and the other was ordinary seed. No difference was found between the initial infection of the cotton

planted on the plots in which the trash had been buried and the ordinary seed. The observation might be of use, because apparently such material was a possible source of infection, and if it could be suitably buried, as was conceivable in certain circumstances, it might be a method of control.

#### DISCUSSION.

MR. BAILEY asked Mr. Hosking if he could clear up a point relating to the development of basal monopodia. It appeared, from what Mr. Hosking had said, that a plant without monopodia would escape the stem lesion phase of the disease, and a better crop would be likely to be obtained from such a plant. But, presumably, the main stem was equally liable to be affected by lesions, in which case the presence of monopodia—any one of which could take the place of the damaged main stem—would be an advantage?

MR. HOSKING said he could not altogether agree, because frequently a very serious infection at the base of the stem killed the monopodia, but the plant nevertheless produced a top crop. This happened even if the stem were affected. If the main stem were killed, naturally the top crop would be killed as well. He did not think, however, that the mortality was more than about 27 per cent. under Uganda conditions. They found that the plants produced some crop if they grew past the basal infection, and if they were non-monopodial, they produced more crop in this way.

MR. BAILEY drew attention to the very remarkable experimental results, obtained by Mr. Massey and his staff, on the effect of flooding in destroying the blackarm organism which persists in the dried-up debris of cotton plants of the previous season.

The experimental plot, which had been liberally covered with fragments of old diseased plants, was sown about the beginning of the third week in August, and careful records were made of the numbers of new-sown cotton plants which had become diseased at different dates. At the end of September, those plots which had received four days floodings showed only 2 per cent. plants infected, whilst the (unflooded) control plots showed about 70 per cent. infection. The reduction was very impressive.

Mr. Bailey also drew attention to another aspect of this experiment, which he considered to be of very great general interest. Within the small compass of a five-acre field, by screening the sub-plots from each other with thick hedges of *Cajanus indicus*, it had been possible to carry out a replicated chequer plot, which had given very clear-cut results, despite the fact that Blackarm was a very infectious disease and might, *prima facie*, have been expected to spread freely from plot to plot, rapidly obliterating any differences between those plots due to original treatments. There was a general feeling that randomised



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field chequer plot technique was not applicable to the study of plant diseases and pests, but the present experiment was an excellent demonstration of the high degree of success which could be obtained.

MR. HOSKING said that in their experiments on Blackarm in Uganda, they had used *Cajanus indicus* between the plots in an attempt to prevent the spread of the disease. This proved quite useless, owing to surface wash, and in subsequent years they had been obliged to dig ditches between the plots. Mr. Hansford's conclusions were that the spread of the disease under Uganda conditions was brought about by surface wash more than by splashing.

In answer to the Chairman, Mr. Hosking replied that cultivation was on the flat and not on ridges, and that the wash was the result of ordinary rainfall, which quite commonly amounted to about  $\frac{1}{2}$  inch in 20 minutes.

MR. MASSEY gave an illustration of Mr. Hansford's observations on the spread of Blackarm in Uganda. He said that on one occasion he had collected a little water from immediately beneath a cotton plant after a heavy rain storm, and found, speaking from memory, 200,000 organisms of *B. malvacearum* per cubic centimetre.

Replying to a question by PROFESSOR BRITON-JONES as to what were the weather conditions in Uganda between two cotton crops, MR. HOSKING said that he could only speak from experience of the Eastern Province. There the cotton was planted between May and the end of August. Normally the rains broke in the middle of March; there was fairly high rain in March, very high rain in April, rain in May, and a lull in June and July; then the second rains began and continued to, roughly, the middle of November. December, January and February were dry, and the peak picking was roughly about Christmas time. In a normal season, where there was no second peak, the cotton could be quite safely uprooted about the beginning of February: under native conditions it was probably uprooted after the break of the rains, which was bad practice, and it was a point that was being watched.

PROFESSOR BRITON-JONES said the reason for his question was that debris was important in the Sudan, whereas in Uganda it was apparently not so important. Mr. Massey thought that if this debris was submerged for about four days under Sudan conditions, infection would be reduced. That might be explained by the fact that, with the introduction of moisture, organisms such as *diplodia* and others—both bacteria and fungi—would tend to swamp *B. malvacearum*. Under dry conditions *B. malvacearum* would remain almost in pure culture. It was well known that a mixture of weed fungi and bacteria tended to kill out the primary parasite. He suggested that that fact might explain the difference between debris in the Sudan and Uganda.

Simultaneously with the extension of the petiole lesions the supply of water and food to the leaf blade is interrupted to an increasing extent, so that the blade gradually withers and dies. As a result of such withering the abscission layer is formed at the junction of petiole and stem. Through this abscission layer the bacteria appear to be unable to penetrate, so that the general process of invasion of the stem tissues via the petiole may be pictured with reasonable accuracy as a race between the progress of the bacterial invasion of the tissues of the petiole and the formation of the abscission layer. Any condition of climate or soil which tends to hasten the formation of the abscission layer will help to reduce the incidence of stem lesions. Among such conditions a few days of dry hot weather are probably the most important, as then the drying out of the leaf blade will be accelerated, as well as that of the petiole itself, which latter will help to retard the rate of bacterial invasion.

In passing, it is worthy of note that an index of what may be termed "potential" stem infections can be obtained quite late in the season by observation of the number of dry leaves still hanging from the stem and branches of the plant. For practical purposes it can be assumed with safety that all these are leaves in which infection has spread from the leaf veins through the petiole tissues. Actual infection of the stem may or may not have occurred, but it is characteristic of this infection process that such leaves tend to remain attached to the plant long after normal senile leaves have been shed.

*Spread of the Disease.*—The process of the development of the stem form of the disease has just been described; it follows from this that the stem form does not spread from plant to plant as such, but rather that an epidemic of the stem form depends on special circumstances, which, fortunately for Uganda, appear to occur comparatively rarely over large areas in any one season. First, the climatic conditions must be such as to ensure a heavy leaf infection on the plants; secondly, conditions must favour the spread of the bacteria through the petioles; thirdly, the leaf blade must not dry out rapidly, otherwise the abscission layer will be formed before invasion of the stem takes place. Most of these conditions obtain during long spells of uninterrupted moist cool weather, but two or three days of hot sunny weather appear to act as sufficient control of the process.

It will be inferred, therefore, that spread of Blackarm, *i.e.* stem disease, may or may not follow on spread of the leaf form (Angular Leaf Spot). The latter is spread by two main agencies: (*a*) wind-blown rain, and (*b*) surface water. The directional spread of the disease is a resultant of the direction of these two agencies, of which the second appears to be the more important under Uganda conditions. During moist weather

the bacteria in the leaf lesions emerge on the surface in the motile state in the film of water on the leaf, and light rain storms driven by wind may transport the bacteria by splashing to neighbouring plants. If, however, as is often the case in Uganda, the wind-blown storms are heavy, the bacteria get washed off the plants into the surface soil water and infection of surrounding plants may not take place. Should the whole of the surface water be absorbed by the soil, spread of the disease may actually be retarded by such storms. On the other hand, during heavy storms, the surface water usually drains off the land, and may run for considerable distances through the cotton fields. Any splashing of this contaminated water by the tail end of the storm on to uninfected plants will leave the bacteria on these under ideal conditions for infection through the stomata of the leaves, stipules and bracts.

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*the seed was correlated with the hair weight, and in many ways was more useful than the ginning outturn, which, as had been said, was very susceptible to small variations which had nothing to do with the case. He did not know whether any of those present remembered Caravonica cotton, with regard to which a claim for a 60 per cent. ginning outturn was made in Queensland ; but the reason for that, as Mr. Thomas had said, was that there was often no inside to the seeds !*

MISS CLEGG asked how pests affected the impurities in the cotton. These impurities chiefly consisted of neps and motes. The most troublesome neps were those which consisted of little tufts that must have arisen in very immature cotton, as a homogeneous mass from the seed itself, and the motes were the broken up immature seeds ; it occurred to her that possibly the problem of impurities was connected to a great extent with the pests which attacked the cotton plant, and it would be of great assistance to them at the Shirley Institute if they could have samples of bolls and raw cotton from the plants which had been attacked by the various pests.

DR. AHMAD, dealing with the effect of insect pest attack on the quality of lint, said that results obtained at the Technological Laboratory at Bombay on cotton that had been attacked by pink bollworm showed that lint from bolls in which all 3 locks had been attacked was definitely inferior in hair properties and spinning quality to that obtained from healthy plants. When only one or two locks had been attacked, the hair weight was appreciably lower, showing lack of available nutrition as a result of the insect pest attack. He suggested that this fact might partly explain the observed anomalies in spinning quality, as other things being equal, finer hairs would tend to produce stronger yarns.

MR. EVELYN asked whether attempts had been made to control Angular Leaf Spot by the method of isolating resistant strains. In St. Vincent this had been done with marked success.

MR. MASSEY replied that this was included in the work that Mr. Trought was doing on the breeding of strains resistant not only to insects, but to Blackarm also.

DR. BUTLER asked Mr. Hosking how the resistant power of U.4/4/2 to Blackarm disease was actually characterised. Were the lesions unable to reach the stem, or did they only reach it and there remain, small and stunted, doing no damage ? He said that one reason for his asking that question was that Findlay had shown that the extent and size of the lesions were greatly influenced, for instance, by water-logging or by ring-barking. Ring-barking reduced the number of lesions compared with controlled plants, while water-logging greatly increased the extent and severity of the lesions. The question he put, therefore, was whether with resistant strains the lesion failed to reach the stem entirely, or reaching it, it got no further.



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MR. HOSKING said the disease failed to produce any lesion in the more resistant types. The figures that he had quoted were percentages of plants showing angular leaf spot infection. There was nothing whatever on the others.

With regard to the effect on quality, MR. ROBERTS asked whether, in the Sudan, they had studied the effects of bad years of disease on the proportion of the various grades of cotton, according to the classification adopted by the Sudan Plantations Syndicate.

DR. F. G. GREGORY said that in Mr. Hansford's paper there were some very interesting remarks on the way in which the bacterium entered and infected the plant. It was stated that infection was primarily produced in the leaf, and that the stem lesions were entirely secondary in their origin. He had seen lesions of Blackarm on the stems of the cotton plants in the Sudan. In the paper it was said that a few days of bright sunshine were sufficient to prevent entirely the infection of the stem, owing to the fact that the leaves were shed. The question then arose, if that were so, how in fact did those stem lesions arise in the Sudan? Firstly, a possible cause of an outbreak of angular leaf spot might be a storm of rain of a few hours, which was followed by an outbreak of Blackarm. In that particular case, the primary infection which in fact happened was followed by a considerable period of very hot and very dry weather; but that did not prevent the stem form of the disease appearing. Secondly, in his recollections of examinations of plants showing the disease, it was often easy to find that the stem lesions occurred very near the apex. At that point, of course, the leaves were not yet expanding, and it became important to ask whether Mr. Hansford's statement should be taken literally as representing the only way in which infection of the plant could take place, or whether in the meristematic portion of the stem, before any woodiness had appeared, primary lesions could not be produced on the stem in the same way as they were in the leaf blade.

MR. HOSKING said that not being a mycologist, he was rather reluctant to answer that question. He thought, however, that Mr. Hansford's meaning was that the method of infection he described in the paper was that which he considered to be the most important. Mr. Hansford had investigated the effect of artificial punctures on the stem in the young plants, and he had reproduced stem lesions. The normal course of events was that the plant was either killed when it was very young, or it survived: if it survived, either it developed Blackarm on its sympodia or on its monopodia, (which reduced the crop) or it did not get Blackarm at all. The amount of damage to the crop really depended (i) on the time of the infection, which varied from year to year, and (ii) what part of the plant was attacked. The greatest loss in Uganda was from the lesions appearing on the sympodia and monopodia, which could nearly always be traced to the leaves and bracts.

DR. BUTLER drew attention to the statement in Mr. Hansford's paper to the effect that some 99 per cent. of the causes of the stem



form of the disease arose from the infection of the leaves or of the bracts. He asked whether this was found in the Sudan also.

- MR. MASSEY replied that the Blackarm type of the disease in infection of the stems and branches might occur in the Sudan at any time during the rainy season, provided that the parasite came to rest and entered young, succulent tissues. The position could be summed up by saying that senescence conferred a high degree of immunity. Before seed disinfection was generally adopted, it was a common sight to see young cotton bearing lesions on all parts of the plant above ground. Nowadays the disease appeared some time in September and was derived from infected debris and infected seedlings present on the adjacent fallows. The conditions then somewhat resembled Uganda, in that the disease was commonly found first of all on young buds and leaves from which it travelled from the petiole to the stem.

It was noteworthy that leaves thickened by Leaf Curl disease were much more difficult to infect than the healthy leaves of the same age and development. The entry of the parasite was largely governed by physical phenomena, e.g., rate of evaporation of the water containing the organism and surface tension.

As regards the disinfection of seed by poisonous dusts, the use of a preparation containing mercury chloride and iodide, for four years during which time over 100,000 sacks of seed had been treated, had resulted in less than six cases of severe dermatitis amongst the natives employed,

The Sudan was interested in a preparation of a less poisonous dust, but he thought that little or no trouble was likely to arise in practice from the small amount of poison adhering to cotton seed.

- MR. BAILEY replied to the question raised by Mr. Roberts as to whether any use was being made in the Sudan of the reports provided by the graders of the Sudan Plantations Syndicate, for the purpose of studying the effect of various diseases on lint quality. Use was being made of these figures. The volume of data was enormous, but it was already clear that the earliness of arrival of the crop had the most outstanding effect on its quality. If the crop came early, the average quality was generally good. Blackarm could affect quality, both directly, by damaging and discolouring the lint, and indirectly, by destroying the earliest-formed bolls, thus throwing the crop late.
- DR. BALLS said he thought there was a chance of confusion arising as between Mr. Roberts' question, and Mr. Bailey's answer, and Dr. Peirce's interests. He believed he was right in saying that the quality classification of the Sudan crop was not a quality classification like that of Alexandria. It was only a classification for grade, whereas the grade (or cleanliness) had not necessarily any connection whatever with the spinning quality. Other things being equal, it usually had, but it was not an inherent and essential connection; it was merely a fortuitous one. Thus the effect of the absence of disease in putting up

## BLACKARM DISEASE IN UGANDA.

*the grade, and therefore giving a higher "quality" cotton as commonly described in the Sudan, did not mean that the spinning quality of that crop was higher. One could get as good yarn out of a low grade lint as out of a high grade one—sometimes!*

MR. BAILEY *did not entirely agree with Dr. Balls. The quality was taken into account to some extent, but, in his opinion, the graders were much handicapped at the present time owing to the grading being carried out on unginned seed cotton. A handful of seed cotton might represent an extremely localised portion of the growing crop, as all the plants which contributed to it were probably growing very close together. It was probably mainly due to this that irregular results were sometimes obtained when the cotton arrived in England.*

MR. SLATER *said that it was not right to define spinning quality in terms of the strength of the yarn only. The amount of dirt and trash in the cotton also had to be taken into account when classifying and evaluating a cotton.*

DR. BUTLER *said that the very interesting discussion of the two papers on Blackarm had shown how necessary it might be to regard the same disease when it occurred in two different localities as, to some extent, a different disease in each place. It was not safe to rely too much on what had been done somewhere else when studying a disease of that kind. He thought that both the Uganda and the Sudan work bore out what he had said. If one were working with a different type of cotton, grown under wholly different conditions, everything in the behaviour of the disease might be different in the two areas, so that for practical purposes it seemed to be really as if one had to do with two distinct diseases. He had seen the peculiar conditions under which the crop was grown in the Sudan; it was a very different proposition in almost every point from cotton cultivation in Uganda.*

## LEAF CURL DISEASE OF COTTON IN THE FIJI ISLANDS.

BY

R. R. ANSON.

Assuming that Leaf Curl of cotton will be one of the subjects brought up for discussion at the forthcoming Conference, I take the opportunity of sending the following short note on our experiences with it in Fiji.

In the season 1931/32, Shambur Sakel and a Sea Island  $\times$  Sakel hybrid, Xh 1029, were tested at the Cotton Experiment Station, and a sharp lookout was kept for signs of Leaf Curl. On an area of one acre three or four suspected plants were rogued out and burnt.

In the current season, three lines of both Sakel and Xh 1029 were planted in a varietal test, together with six families of New Guinea Backcrosses, Sea Island, Sea Island white flower, New Guinea closed flower type, three New Guinea  $\times$  Sea Island double backcrosses and an isolated green Kidney type from Trinidad. Early in February signs of Leaf Curl broke out in the Shambur Sakel, spread to Xh 1029 hybrid, and subsequently to Sea Island and some of the double backcrosses. As soon as this was observed, all infected plants were immediately uprooted and burnt.

It is possible that this may not be the same Leaf Curl as that alluded to by R. E. Massey and F. W. Andrews in their preliminary note on "Leaf Curl Disease of Cotton in the Sudan" (Empire Cotton Growing Review, Vol. IX, No. 1). The symptoms seem the same, and no such disease has ever been noticed on cotton in Fiji before.

This disease would appear to have been transmitted from plant to plant by wind-blown rain, but it is just possible that jassid may have operated as a vector, although its attack at the moment is very slight.

If, however, this is one and the same disease as is described by Massey, Andrews and Golding ("A Vector of Leaf Curl of Cotton in Southern Nigeria"), it would appear that the disease is spread by infected seed and subsequently either by a vector or wind-blown rain, the soil having nothing to do with it. This being the case, one would imagine that complete control might be brought about by destroying all host plants and the introduction and complete isolation of healthy seed from unaffected regions.

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THE CHAIRMAN mentioned that Mr. Anson had followed up his note on this subject with a coloured illustration of the disease about which he would be glad to receive any information, particularly as to whether it was the same disease as that from which cotton suffered in the Sudan and in certain other countries. The coloured drawings had been circulated earlier in the Conference.

#### DISCUSSION.

*In answer to the question put by the Chairman as to whether the disease depicted was the same as leaf curl disease of cotton in the Sudan, MR. MASSEY gave it as his opinion that judging from the illustrations, it was not at all similar.*

PROFESSOR BRITON-JONES said that the illustrations suggested to him a similar disease in apples, which was caused by a deficiency of potash in the soil.

MR. THOMAS suggested that the disease was similar to that which they called Leaf Curl in the Punjab, which was associated with jassid attack. He asked whether it was similar to the effect produced by jassid in South Africa. From information he had recently received, he had come to the conclusion that the leaf curl associated with White Fly in the Sudan was different from that which they called Leaf Curl in the Punjab. White Fly in the Punjab was far more prevalent than it was in the Sudan, but he had never been able to associate their form of Leaf Curl with that pest, and this was confirmed by the Entomologist at Lyallpur: he definitely associated their outbreaks of leaf curl with jassid. He suggested that in these circumstances, their disease in the Punjab, which might be the same as that experienced in Fiji, should be given a different name. He also asked whether Leaf Curl appeared on American cotton as well as on Sakel cotton in the Sudan.

DR. BUTLER said he thought it was possible to have the same set of symptoms arising from several different causes. The leaf curl as it occurred in the Sudan was like malaria, requiring both the insect and the parasite to produce the disease; and the position in the Punjab and other countries where the White Fly occurred but not leaf curl, was exactly the same as in England, where the *Anopheles* mosquito occurred but no malaria, because the *Anopheles* had not got the parasite in it. The Sudan leaf curl was a definitely parasitic disease carried by the White Fly, but there appeared to be various other types of leaf curl, some of which were associated with red leaf. He agreed that red leaf was a symptom of various disorders; he had seen it frequently in India, and it could be attributed to a number of different conditions: he did not think they were generally worth a separate name. If due to potash deficiency, it was better to call it potash deficiency and not leaf curl or red leaf, which were just symptoms. In the Sudan disease, however, the cause was known to be a virus and a specific name was necessary. That selected might not be the best name. "Crinkle" and "leaf roll" had been suggested, but had the disadvantage that they had already been used for other cotton diseases. "Leaf curl" was first applied to this virus disease, and therefore one was more or less obliged to adopt the name as originally used in Nigeria and then in the Sudan, in spite of the fact that there were other things that curled leaves.

MR. JACKSON said that he was inclined to think that the phenomena referred to by Mr. Thomas were similar to those found on the black soils in Central India and in the Central Provinces. They there called it Leaf Roll, and it was usually associated with red leaf. In the typical form of red leaf, the leaf curled downwards, and the upper surface



became convex, whereas in the disease known as leaf curl in the Sudan, he understood that the leaf curled upwards. In his view, leaf roll was a physiological effect not associated with any insect pest, though an insect might be attracted by the sap condition arising out of the physiological state of the plant. It was not a communicable disease, as found at Indore, and they had been able to produce it at will by manipulating soil conditions.

MR. PARNELL, answering Mr. Thomas' question as to whether the effect of jassid in South Africa was similar to the disease in Fiji, as shown in the illustrations, replied that on the whole he thought that this was something different. He would like to add, however, that the effect of jassid on different types of cotton varied considerably, and that in view of the fact that the disease in Fiji occurred on Sakel  $\times$  Sea Island hybrids—a type of cotton with which he was not familiar—he could not be positive, but the illustrations did not suggest that the damage was due to jassid.

MR. LEWIN said that he believed that Leaf Curl was first discovered in Nigeria and was attributed to the White Fly by Golding in 1929. The drawings submitted by Mr. Anson in no way conveyed to him the characteristic symptoms of the disease as it occurred in Nigeria, except possibly the drawing of the healthy boll. If in that illustration the vein had been shown slightly less green than the rest of the boll, it would have been a good illustration of a boll attacked by what they called Leaf Curl in Nigeria. In the disease as found in that country, the symptoms on the leaves were very characteristic; there was a certain distortion and a varying degree of mottling, but the most characteristic symptom was the glossiness on the inside of the leaf—mainly on the vein. The disease Leaf Roll was, he believed, first discovered by Mason and Jones in 1923. It was chiefly a disease of Upland cotton in Nigeria, and the symptoms were as those described by Mr. Jackson. Leaf Roll was decided in Nigeria to be due entirely to physiological causes, and it was found that it could be produced by subjecting a healthy plant to artificially increased atmospheric pressure. On the other hand, Leaf Curl was a virus disease, transmitted by White Fly, but neither disease in Nigeria resembled in their symptoms the drawings submitted by Mr. Anson.

With a view to throwing further light on the possibility of the disease in Fiji being due to jassid, the Chairman asked Dr. Tothill whether jassids occurred in Fiji, to which he replied that the insect certainly did occur there. DR. TOTHILL referred also to the suggestion that the disease might be due to potash deficiency, and said that if this were so, it was not clear why cotton had not suffered from Leaf Curl in the preceding years, seeing that the crop had been under continuous cultivation since the revival of cotton-growing in 1924. The Chairman repeated that the disease did appear to be specific and to need further investigation.



# BOTANICAL PROBLEMS OF THE COTTON PLANT.

*Chairman :* PROFESSOR V. H. BLACKMAN, F.R.S.

The following papers were presented and were discussed together :  
 "ENVIRONMENT AND HEALTH IN THE COTTON PLANT" and "SOME  
 OUTSTANDING PHYSIOLOGICAL PROBLEMS IN THE CULTURE OF  
 COTTON IN THE SUDAN."

## THE HEALTH AND VIGOUR OF THE COTTON PLANT IN RELATION TO ITS ENVIRONMENT.

BY

F. K. JACKSON (*Institute of Plant Industry, Indore*).

At the Institute of Plant Industry, Indore, the improvement of cotton forms one of the main items. Except for about 970 square miles irrigated by the Gang Canal (a Sutlej scheme) in Bikaner State, and a limited acreage scattered in "garden" lands in Malwa and Rajputana, the fate of the cotton crops is dependent on the vagaries of the monsoon rains. The crops grown as a rule are varying mixtures of low-yielding Old World strains.

TABLE I.

VARIATION IN SOIL AND CLIMATE IN AREAS SERVED BY THE INSTITUTE OF PLANT INDUSTRY.

|                                                      | <i>Bikaner.</i> | <i>Jodhpur.</i>             | <i>Jaipur.</i> | <i>Malwa.</i>              | <i>Nimar.</i>             |
|------------------------------------------------------|-----------------|-----------------------------|----------------|----------------------------|---------------------------|
| Total Rainfall in inches ... ..                      | 11.79           | 13.62                       | 24.21          | 33.52                      | 20.0                      |
| Mean Temperature during Cropping Season ... ..       | 78.4            | 77.2                        | 75.6           | 72.5                       | 75.5                      |
| Mean Humidity during growing Season Per cent. ... .. | 58              | 55                          | 63             | 71                         | 68                        |
| Soil Type ... ..                                     | Sandy Loam.     | Sandy Loam and Clayey Loam. | Sandy Loam.    | Plateau Type Black Cotton. | Valley Type Black Cotton. |

Among other activities a search is being made for the causes underlying the generally very low and uncertain yields of cotton in the 15,000 square miles of land on which this crop is grown in the area served by the Institute.

These low yields can only be partly attributed to disease—notably root rot in Bikaner, wilt in Nimar (a tract some 1,200 feet lower than the Malwa plateau), and leaf roll and red leaf of New World varieties in all districts.

Furthermore, these low yields do not seem to be due to any intrinsic soil deficiency shown by analysis, and they vary enormously from field to field and season to season. Even in the better yielding tracts yields are depressed by persistent, heavy monsoon rains as in 1933 in Malwa. In that year similar lowering of yield took place even in the irrigated desert of Bikaner where a rather lower temperature and an unusual atmospheric humidity were the chief expressions of the monsoon. The intensity of shedding rose when dryness succeeded such humidity. Is water-supply or a nutritional deficiency responsible? The accumulation of nitrates in soils appears to be low under humid conditions; they become locked up in algæ which then grow vigorously in normal soils. It is still lower if salts are accumulated in the surface zone, which invariably occurs with the advancing season. Nitrification is very active at high temperatures which perhaps partially explains the high yields in canal areas, such as the Punjab, where the crop begins in the hot season.

Yields have also been found to be seriously depressed if sowing is delayed long after the onset of the rains and, conversely, much increased if sown only two or three weeks before they break.

TABLE II.  
YIELD DIFFERENCES BETWEEN HOT-WEATHER AND RAIN-SOWN CROPS OF COTTON.

| Place.      | Year.   | Cambodia. |            | Malhi.    |            |
|-------------|---------|-----------|------------|-----------|------------|
|             |         | Pre-Sown. | Rain-Sown. | Pre-Sown. | Rain-Sown. |
| Jodhpur ... | 1933-34 | 980 lbs.  | 802 lbs.   | 988 lbs.  | 737 lbs.   |
| Jaipur ...  | 1932-33 | 1628 "    | 222 "      | 1798 "    | 890 "      |
|             | 1933-34 | *821 "    | 231 "      | 545 "     | 494 "      |
| Indore ...  | 1932-33 | *482 "    | 280 "      | 1557 "    | 818 "      |
|             | 1933-34 | *821 "    | 432 "      | 907 "     | 875 "      |
| Dhar ...    | 1933-34 | *1270 "   | 713 "      | 1491 "    | 1154 "     |

\* These are post-rain irrigated.

There is no very great fall in the mean maximum temperature once the rains have broken (in June usually) and, in fact, the daily fluctuations are least during the monsoon. The cause of low yields due to late sowing seems hardly likely to be found in temperature.

Most of the evidence points to low yields being due to *nutritional deficiency*, which may be brought about in various ways.

## THE HEALTH AND VIGOUR OF THE COTTON PLANT.

In manurial experiments on the black cotton soil at Indore, surface dressings of compost or farmyard manure gave no significant increase in the yield of cotton (A, Table III), but where compost was drilled along with the seed a higher yield and better boll development was obtained (B). Compost applied throughout the profile to a depth of over two feet gave yet more decisive results—the deeper soil and other conditions exerted a great influence on yield and boll development (C).

TABLE III.

INFLUENCE OF MANURING AND SOIL TEXTURE ON THE YIELDS OF COTTON IN MALWA.

## A

VARIETY—MALWI BULK.

Lbs. Per Acre.

| <i>Field No.</i> | <i>Untreated.</i> | <i>Surface Dressing of Compost.</i> |
|------------------|-------------------|-------------------------------------|
| 45 I.P.I. ... .. | 441               | 482                                 |
| 11 „ ... ..      | 245               | 327                                 |

## B

VARIETY—MALWI 9.

Average Yield in Gms.

|                           | <i>Compost Drilled in at Sowing Time.</i> | <i>Surface Dressing of Compost.</i> |
|---------------------------|-------------------------------------------|-------------------------------------|
| Seed Cotton per Plant ... | 5.64                                      | 3.84                                |
| Seed Cotton per Boll ...  | 1.16                                      | 1.09                                |

## C

VARIETY—MALWI 9.

Average Yield in Gms.

|                           | <i>Untreated.</i> | <i>Manuring Throughout the Profile with Compost.</i> |
|---------------------------|-------------------|------------------------------------------------------|
| Seed Cotton per Plant ... | 4.7               | 10.2                                                 |
| Seed Cotton per Boll ...  | 1.11              | 1.18                                                 |

## D

AVERAGE YIELD IN GMS.

| <i>Treatment.</i>                                                       | <i>Seed Cotton per Plant.</i> |                 | <i>Seed Cotton per Boll.</i> |                 |
|-------------------------------------------------------------------------|-------------------------------|-----------------|------------------------------|-----------------|
|                                                                         | <i>Indore 1.</i>              | <i>Mahwi 9.</i> | <i>Indore 1.</i>             | <i>Mahwi 9.</i> |
| 1. Heated Soil mixed Half and Half up to 6 inches without Manure ... .. | 22.1                          | 17.6            | 2.40                         | 1.43            |
| 2. No Heated Soil no Manure ... ..                                      | 8.04                          | 10.87           | 2.01                         | 1.07            |
| 3. Heated Soil as above with F.Y.M. ...                                 | 34.74                         | 19.41           | 2.22                         | 1.18            |
| 4. No Heated Soil with F.Y.M. ... ..                                    | 10.06                         | 15.03           | 2.01                         | 1.09            |

Remarkably interesting results came from an experiment where the upper six inches of soil were removed, half of it lightly fired to de-hydrate the colloids, mixed with the other half and returned to the plot. This treatment was expected to prevent or retard the deterioration of tilth which usually occurs on the black soils in Malwa after the rains break. Large increases, both in yield and boll-development, were again secured and when compost was also applied the yield of Indore 1 (a Cambodia selection) was actually quadrupled (D). On examining the soil profiles it was found that up to a depth of about eighteen inches the soil was much looser in texture than that in the controls. It was clear that by itself open texture influenced considerably both yield and boll-development and that farmyard manure was far more efficacious when associated with open texture. Confirmation of this is found in the soils of certain villages in Malwa, well known for their high quality cotton, whose soils are of markedly more open texture than those of other areas. These facts emphasize the need for free movement of moisture and air in the root-region of the cotton plant and consequently the need for devising practical means for better soil management, so that, while climatic factors are beyond control, the plant may be enabled to react to them successfully. It is hoped to find out whether the better development of bolls in these experiments is associated with fewer immature hairs, and thus with decreased "neppiness."

*Leaf-roll.*—Cambodia cotton when grown on the heavy black cotton soil in Malwa develops leaf-roll, one of the two obscure diseases described by Mason.\* Even in the light sandy soil of Jaipur the same leaf-roll appeared last year in some fields. An examination of soils showed differences in the capillary rise and rates of percolation.

TABLE IV.

MOBILITY OF WATER IN THE SANDY SOILS OF JAIPUR.

|                                            |                | <i>Good Soil.</i> | <i>Bad Soil.</i> |
|--------------------------------------------|----------------|-------------------|------------------|
| Rate of Capillary Rise per 4 ins. ....     | 0-6 ins.       | 32 Minutes        | 6 Minutes        |
| " " " " " " " " " " " " " " " "            | 6 ins.-15 ins. | 3                 | 6                |
| Percolation "per hour" " " " " " " " " " " | 0-6 ins.       | 25-5 cc.          | 78-9 "cc."       |
| " " " " " " " " " " " " " " " "            | 6 ins.-15 ins. | 105-8 cc.         | 30-2 cc.         |
| Per cent. drop "at the end of three days"  | 6 ins.-15 ins. | 40-8              | 13-6             |
| Total Solids " " " " " " " " " " " "       | 0 ins.-6 ins.  | 0-048             | 0-026            |
| " " " " " " " " " " " " " " " "            | 6 ins.-15 ins. | 0-042             | 0-034            |

On a comparison of root development of several pairs of healthy and diseased plants standing side by side within a distance of 14 inches it was noticed that the root activity, especially in the

\* *Annals of Botany*, Vol. XL, 1926, p. 765.

## THE HEALTH AND VIGOUR OF THE COTTON PLANT.

subsoil was considerably less in the diseased plants. For example the total length of active roots on a healthy plant may be nearly six times that of a closely adjacent diseased plant.

The soils around the diseased plants showed in general higher salt contents, as indicated by greater osmotic pressures up to a depth of at least 15 inches.

TABLE V.  
OSMOTIC PRESSURES OF HEALTHY AND UNHEALTHY LEAVES OF CAMBODIA COTTON, INDRE I AND OF CORRESPONDING SOILS.

|                                                                            | Healthy. | Unhealthy. | N = | T =   | P.     | Significance.                 |
|----------------------------------------------------------------------------|----------|------------|-----|-------|--------|-------------------------------|
| Leaves... ..                                                               | 6.7      | 5.2        | 120 | 6.87  | > 0.01 | Very Significant.             |
| Soils ... 0 ins.- 3 ins.                                                   | 3.59     | 13.37      | 10  | 4.940 | > 0.01 | "Distinctly "<br>Significant. |
| 12 ins.-15 ins.                                                            | 10.37    | 12.22      | 10  | 3.434 | > 0.01 |                               |
| Per cent. Drop of Osmotic pressure from Lower to Upper Zone of Soil ... .. | 64.27    | 29.44      | 10  | 3.842 | < 0.01 | Distinctly Significant.       |

The drop in salt concentration from the lower zone to the surface was very much less in the root zone of the diseased plants. On examining the plants, the leaf-sap from those diseased was more acidic and dilute than the sap of healthy ones. Leaf-roll could be artificially produced in pot culture when the surface soil was kneaded by hand; on the other hand perfectly healthy Cambodia plants were grown in 1933 in plots treated with lightly fired soil as described above.

*Fusarium wilt*.—Recently, while comparing the root development of wilted and non-wilted plants a similar reduction of activity and an increased proportion of dead roots were found. It appears that the root activity in the first foot of soil profile considerably affects the plants. In the first nine-inch layer the ratio of "active" to "dead" roots in a healthy cotton plant was 3:1 whereas in an adjacent wilted plant this ratio was 0.6:1. Mere presence of fungus does not seem to be enough to cause wilting, a certain reduction in root activity is essential. The rôle of *fusarium* in cotton wilt still requires further elucidation. How far pre-disposition of the plant is necessary for a successful attack is not yet clear. This is really a question of what disease resistance means. The virulence of the attack appears to be periodic. (See Fig. 1.)

Under the same conditions, the behaviour of different varieties seems to vary. The increase in salt content referred to depends on decreased soil permeability.



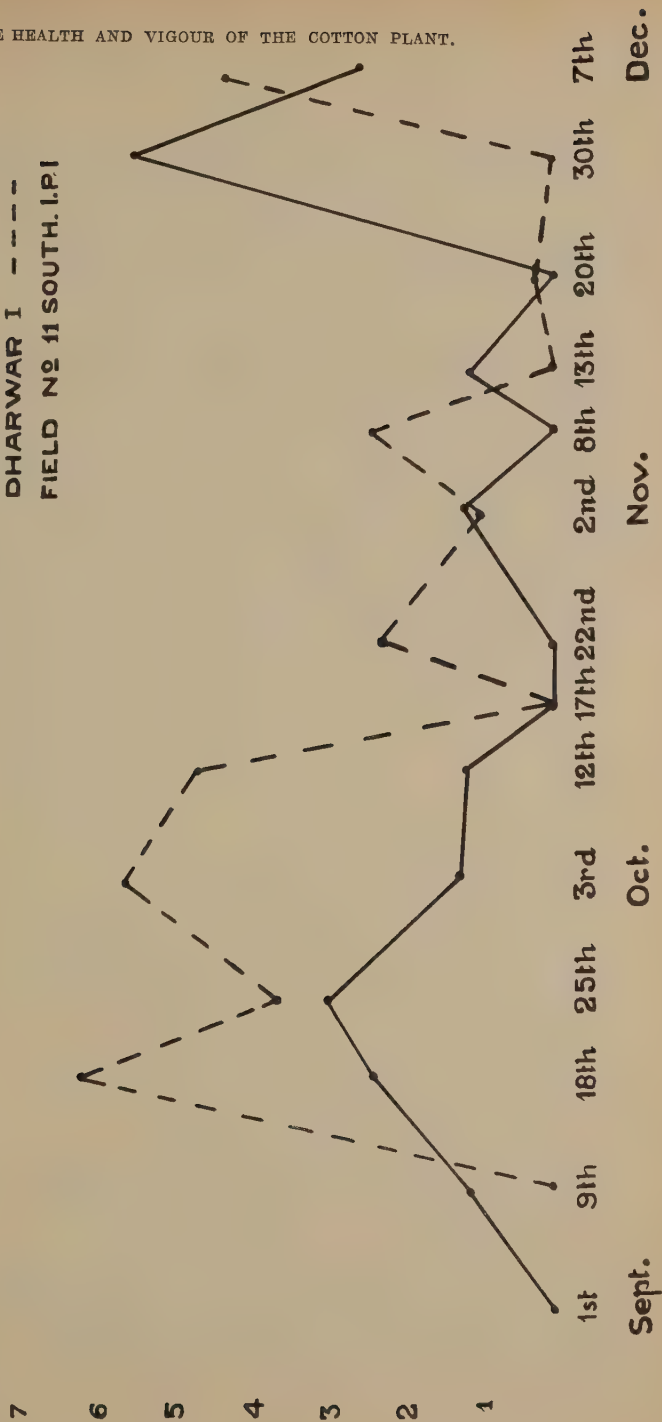
*Root-rot.*—A comparison of soils from patches where severe attack of root-rot of cotton occurred, with other parts of the field carrying healthy plants, showed a development of comparatively anærobic conditions where root-rot occurred. The soil formed hard lumps, and humus separated into black streaks irregularly distributed.

Work is still in progress on most of the points raised in this paper and the comments and suggestions of other workers are invited. The results described seem to indicate an urgent need for a very close study of soil changes during crop growth and their influence on plant metabolism. Probably the differential behaviour of roots under changing conditions will be a useful method for that accurate study, provided quantitative methods are developed. Such studies are obviously of a complex nature and demand simultaneous attack from several aspects. Only a very close co-ordination of workers well-trained in the various sciences is likely to achieve the end desired.

# PERIODIC VIRULENCE OF WILT

PERCENT

VERUM -----  
DHARWAR I - - - -  
FIELD NO 11 SOUTH I.P.I



THE HEALTH AND VIGOUR OF THE COTTON PLANT.

MR. F. K. JACKSON said that although his name appeared as the author of the paper, the work had been done by his colleagues, notably Mr. J. D. Wad, Chemist and Agronomist at Indore.

Mr. Jackson proceeded to illustrate by diagrams some of the principal points in his paper.

## SOME OUTSTANDING PHYSIOLOGICAL PROBLEMS IN THE CULTURE OF COTTON IN THE SUDAN.

BY

F. G. GREGORY (*Imperial College of Science and Technology*).

At the last Conference in 1930 a paper was read presenting the yield data obtained in a multiple-factor experiment carried out at the Gezira Research Farm, Sudan, in the season 1929-30. The experiment was repeated in 1930-31, and the results of both these experiments have since been published (*Journ. Agr. Sci. XXII* : pp. 617-638, 1932). The mean yields of cotton obtained in the two years were as follows :—

TABLE I.

|         |        | <i>Four factor experiment.</i> | <i>Gezira yield.</i> |
|---------|--------|--------------------------------|----------------------|
| 1929-30 | ... .. | 2.08 kantars per feddan        | 2.12 k.p.f.          |
| 1930-31 | ... .. | 1.85   "   "   "               | 1.29   "             |

For comparison with the experimental yields the mean yields for the whole Gezira area under irrigation are included. Almost identical values are seen in both years, and therefore the data obtained from the experiments may be regarded as representative of the whole irrigated area. These results exemplify the main problem of cotton culture in the Sudan, namely, the large annual variations in yield.

Various suggestions have been put forward to account for these fluctuations in yield, which will be considered under the following headings :—

1. The incidence of pests and diseases ;

2. Climatic effects (a) directly on the development of the plant, (b) indirectly by bud and boll shedding, (c) indirectly through the action of rain on the soil.

1. As to the first suggestion, sufficient evidence has accumulated to show that the effect of disease and pests is secondary only, and not the primary cause of the fluctuations from year to year.

## PHYSIOLOGICAL PROBLEMS OF THE COTTON PLANT.

2. With regard to the climatic effects evidence is to hand on each of the points raised.

2 (a). The climatic conditions in the two years may be summarised from the statements made by Mr. Lambert in the annual reports of the Gezira Research Farm. In 1930 he states "The whole season has been quite abnormal." Temperatures and humidities were all below normal. "The conditions would appear to have been exceptionally favourable to growth." A low yield, however, was obtained both on the farm and over the whole Gezira area. In 1930-31 he reports: "Meteorological conditions very normal and would be expected to be favourable for plant development. Early rainfall in May and June." An abnormally low yield was everywhere recorded in this season. Climatic variation between the two years under review was, therefore, considerable. If climatic conditions were of major importance, it is clear that in two such different years different effects would be expected on plants of different sowing dates, since the later sown plants were not exposed to the direct effects of weather in the earlier parts of the growing season.

In this connection the relative yields for the two years 1929-30 and 1930-31 in the four-factor experiments are illuminating. They are given in the Table below.

TABLE II.

| <i>Sowing dates.</i>    | <i>July 22, 24.</i> | <i>Aug. 11, 12.</i> | <i>Sept. 2.</i> | <i>Sept. 23, 25.</i> |
|-------------------------|---------------------|---------------------|-----------------|----------------------|
| Control plots ... ..    | 75.1                | 61.4                | 57.0            | 58.9                 |
| Nitrogen addition... .. | 65.6                | 67.9                | 66.7            | 68.4                 |

The figures represent the average yields of all the treatments for the various sowing dates in 1930-31 as percentages of values for identical treatments and sowing dates in 1929-30. So far as the manured plots are concerned, there has been no differential effect of the climatic conditions among the sowing dates. The climate in the early part of the season (between July and September) cannot therefore have been of importance in determining the yields of the manured plots, nor can the low yields in the second year be accounted for in this way. After September the plants of various sowing dates were indeed exposed to identical climatic conditions in each of the years, but the plants of different sowing dates were at different stages of development, and yet no differential effects of climate are found.

The developmental curves support the evidence of relative yield. Comparing the flowering curves for the two years it is seen that up to the end of November the rate of flower production is identical; after this, however, a rapid divergence appears, the values for 1930-31

falling rapidly behind (G.R.F. Rep., 1930-31). The rate of development of the plants was thus identical, and this is confirmed by the rate of formation of nodes on the main stem, which is again identical up to the end of December. As far as development is concerned, the evidence goes to show that climatic conditions were not the controlling factor.

2 (b). The cause of bud and boll shedding is still by no means clear. It is generally attributed to water-strain. The two-factor experiment in the Gezira (1928-29) showed, however, a constant percentage of boll shedding in all combinations of water and nitrogen supply. The cause is probably internal and due primarily to nitrogen shortage and competition between growing parts.

An experiment performed in 1932-33 in which flower-buds were artificially removed is decisive in this matter. Flower buds were removed on three separate occasions, viz., Oct. 17, 31, Nov. 15. The yields of the plants were, however, increased by such removal, except in the one case in which all buds were completely removed until Nov. 15. Early removal of buds had no detrimental effect on yield. Late boll shedding will reflect mainly the internal nitrogen-starvation level in the plants. Thus in 1930-31 there was excessive late boll-shedding, but as will be seen, in this year nitrogen-starvation in all treatments was acute.

Finally, the following figures of yield and shedding are pertinent :—

TABLE III.

|         |     |     |     |     | <i>Per cent. flowers lost as buds and bolls.</i> | <i>Gezira yield.</i> |
|---------|-----|-----|-----|-----|--------------------------------------------------|----------------------|
| 1927-28 | ... | ... | ... | ... | 45 per cent.                                     | 3.29 k.p.f.          |
| 1928-29 | ... | ... | ... | ... | 38 „ „                                           | 3.54 „               |
| 1929-30 | ... | ... | ... | ... | 33 „ „                                           | 2.12 „               |
| 1930-31 | ... | ... | ... | ... | 48 „ „                                           | 1.29 „               |

2 (c). The action of rain in causing waterlogging of the soil has been regarded as a major cause of low yields. Again, direct experimentation has failed to confirm this. In 1932-33 an experiment with deliberate waterlogging was carried out. Heavy early waterlogging before sowing failed to have any significant effect on yield. Late waterlogging for seven days after Sept. 16 definitely reduced yield : such a late waterlogging is unlikely to occur in normal practice. At three weeks old, waterlogging for seven days produced marked symptoms in the plants. The leaves became yellow, the nitrogen content fell rapidly. No leaf shedding occurred, but the roots were partially destroyed. During waterlogging leaf-growth ceased, but stem-growth continued, and carbon assimilation also had taken place during the period. Rapid recovery takes place after removal of the water, and



good yields have been obtained. (Hag Abdulla Outstation experiment. G.R.F. Report, 1932-33.)

The effect of heavy rainfall at sowing or immediately afterwards is probably due to the poor germination, loss of plants leading to irregular spacing, and late re-sowing at too wide a spacing : late sowing and wide spacing in combination lead inevitably to low yields. The factors so far considered cannot therefore have been instrumental in producing the low yield in 1930-31.

### *The Nitrogen Problem.*

The introduction of the sampling method, in which plants at regular intervals are removed, the various parts weighed and subsequently analysed, has made possible an adequate analysis of the seasonal variation in yield. The causal nexus may be briefly described as follows. In the very early stages of growth the number of growing points and organs is small, so that the nitrogen supply from the seed and soil is adequate and development is controlled by external factors. As the number of growing points increases the rate of the utilisation of nitrogen within the plant overtakes the supply so that the nitrogen content, after reaching a maximum, begins to fall. This phase is one of increasing internal nitrogen starvation, and during it the nitrogen content is a measure of the rate of supply from the soil. Development is still partly determined by the level of external factors. When the level of nitrogen content has fallen to approximately 2.8 per cent. of the dry weight, the rate of development is conditioned completely by the internal nitrogen level. This point antedates very closely the point of maximum leaf area or weight of the plant. The point of maximum rate of increase in leaf weight coincides in time with the maximum rate of flowering. After this phase in development boll-formation begins to be active and rapid transference of nitrogen takes place from the leaves to the developing bolls. Leaves thus die off, and the time of maximum rate of boll formation exactly corresponds with the time of maximum weight of leaves. At the time of maximum boll-formation the uptake of nitrogen falls to zero. The reason for this is not quite clear, but presumably it is caused by the cessation of growth of roots consequent on diversion of carbohydrates to the bolls. Be this as it may, at this point the maximum nitrogen content of the plant has been reached. The leaf growth rate is conditioned almost entirely by the rate of supply of nitrogen, and thus the maximum leaf-weight is a measure of the total nitrogen taken up by the plant. Ample evidence exists (G.R.F. Report, 1930-31, fig. 4) to show that the maximum leaf weight is highly correlated with final yield, and indeed this relation has been used successfully to forecast yields in the Sudan before picking of cotton has commenced.

A comparison of the leaf weights of plants of the various sowing dates in 1929-30, 1930-31 is very informative (G.R.F. Report, 1930-31, fig. 3). At all sowing dates with and without nitrogen addition the maximum weight of leaves is much less in 1930-31 than in the previous year.

Figures for nitrogen content of the green leaves present interesting features, and are given in Table V. for the year 1929-30, for the treatment giving maximal yields (heavy watering and close spacing).

TABLE V.

| Sowing dates.        | Controls. |      |      |      | Nitrogen addition. |      |      |      |
|----------------------|-----------|------|------|------|--------------------|------|------|------|
|                      | I.        | II.  | III. | IV.  | I.                 | II.  | III. | IV.  |
| Sampling dates :—    |           |      |      |      |                    |      |      |      |
| Aug. 16 ... ..       | 3.30      | —    | —    | —    | —                  | —    | —    | —    |
| Sept. 5 ... ..       | 3.72      | 3.20 | —    | —    | —                  | —    | —    | —    |
| Sept. 26 ... ..      | 3.78      | 4.00 | 3.60 | —    | 4.26               | 4.18 | —    | —    |
| Oct. 15 ... ..       | 3.80      | 4.46 | 4.48 | 3.52 | 4.52               | 4.46 | 4.54 | —    |
| Oct. 30 ... ..       | 3.10      | 3.40 | 3.84 | 3.84 | 3.74               | 3.64 | 4.00 | 3.94 |
| Nov. 14 ... ..       | 2.44      | 2.60 | 3.06 | 3.50 | 3.24               | 3.20 | 3.62 | 3.52 |
| Nov. 29 ... ..       | 2.02      | 2.16 | 2.70 | 2.98 | 2.78               | 2.60 | 3.34 | 3.26 |
| Dec. 14 ... ..       | 1.74      | 1.96 | 2.16 | 2.62 | 2.62               | 2.68 | 2.98 | 2.96 |
| Dec. 29 ... ..       | 1.58      | 1.66 | 1.90 | 2.24 | 2.38               | 2.36 | 2.88 | 2.96 |
| Jan. 13, 1930 ... .. | 1.86      | 1.64 | 1.72 | 2.24 | 2.36               | 2.38 | 2.68 | 2.90 |
| Jan. 28 ... ..       | —         | 1.56 | 1.76 | 2.06 | —                  | 2.24 | 2.38 | 2.78 |
| Feb. 12 ... ..       | —         | —    | 1.78 | 1.86 | —                  | —    | 2.44 | 2.82 |
| Feb. 27 ... ..       | —         | —    | —    | 1.96 | —                  | —    | —    | 2.66 |
| Means ... ..         | 2.73      | 2.66 | 2.70 | 2.68 | 3.24               | 3.08 | 3.21 | 3.09 |

Sowing dates: I, July 22; II, August 11; III, September 2; IV, September 25.

The following facts may be seen from the table :

(1) In both controls and manured plots the nitrogen-content rises at first and reaches a maximum value on the same date (Oct. 15) irrespective of sowing date.

(2) The absolute maximum nitrogen content is in the neighbourhood of 4.5 per cent. in both control and manured plants.

(3) In the control series the plants of sowing dates I and IV never reach the absolute maximum nitrogen content. The yield relations are exactly similar, as may be seen in Table VI.

TABLE VI.  
YIELDS IN KANTARS PER FEDDAN.

| Sowing dates.            | I.    | II.   | III.  | IV.   |
|--------------------------|-------|-------|-------|-------|
| Control series ... ..    | 1.318 | 1.889 | 1.798 | 1.428 |
| Nitrogen addition ... .. | 2.773 | 3.077 | 2.664 | 1.735 |

(4) In the manured series of the fourth sowing date the maximum nitrogen content falls considerably below that of the other sowing dates : a similar relation holds for yield.

(5) The nitrogen content of sowing date IV for controls and manured plants is not very different in the early stages : the yields also do not differ much (Table VI).

The yields of treatments within one year are thus seen to be closely related to the nitrogen relations of the leaves.

Comparing such figures for the two years as are available (G.R.F. Report, 1930-31, fig. 6), it is seen that in the year of lower yield (1930-31) the nitrogen content of the leaves never rises to the maximal value found in 1929-30 (highest value 4 per cent.). This lower level of nitrogen content is reflected in the lower growth rate of the leaves, and hence of the whole plant (G.R.F. Report, 1931-32, fig. 9). All the findings in the two experiments mentioned are confirmed by a later sowing-date experiment in 1931-32 (G.R.F. Report, 1931-32). Much higher yields were obtained in this year (average 3.2 k.p.f.) and the plants showed higher levels of nitrogen content, and these high levels were longer maintained. In consequence, leaf growth remained high for a longer period, giving maximum weights of leaf twice as high as in 1930-31 (G.R.F. Report, 1931-32, fig. 10). The fluctuating yields in the years considered are thus completely explicable on the basis of nitrogen supply and content of the plants.

Referring once more to Table V, it is seen that the maximum nitrogen content occurs on Oct. 15. This date corresponds with the maximum nitrate content found in Gezira soil. The rising content of the leaves of the early sowing dates reflects no doubt the rising level of nitrate in the soils : the uptake rate is proportional to the concentration.

The fact that maximum nitrogen content of the leaves never rises beyond 4.5 per cent. is of interest. Only a limited amount of nitrogen can be absorbed by the plant, however high the supply may be, and at any stage is largely controlled by the number of leaves present. In the last sowing date the maximum is never reached, though the manured plants have abundant nitrogen present in the soil. Maximal nitrogen uptake for sowing dates III and IV occurs on Nov. 29 (G.R.F. Report, 1930-31, fig. 6), at which time the nitrogen content of the leaves has fallen to 3.3 per cent. It would appear that the maximum nitrogen content of the leaves at any calendar date is determined by some external factor. The maximal values at different dates show an optimum relation, which rises from July to reach a maximum in mid October and then falls again. This corresponds precisely with the temperature curve in the Sudan and opens up an interesting possibility in the study of the nitrogen metabolism of the plants. Sufficient has been said to substantiate the thesis that nitrogen supply is the main controlling factor in the growth of cotton in the Sudan. Yearly

fluctuations in nitrogen supply must occur. This is a major problem for the soil chemists. Little at present is known. The only suggestion which is confirmed by facts is that put forward by Dr. E. M. Crowther in 1925, namely, the detrimental effect of May and June rainfall on the subsequent crop. The year 1930-31 was in fact characterised by abnormally heavy rains in May and June. The evidence from the study of the plant indubitably points in this direction.

*The Problem of late-sown Cotton.*

One of the most interesting results from the sowing date experiments was the comparative failure of late-sown cotton in all treatments. These plants are characterised by having little lateral sympodial development and, hence, few flowers. The reason for this is not at all clear. Table VI shows further that these late sown plants show little benefit from added nitrogen. Two possibilities are—(1) The ammonium sulphate added is not available. (2) There is a failure on the part of the plant to take up the nitrogen.

In Table V the average nitrogen contents of the leaves of the plants are given. It is seen that the late-sown plants have on the average as high a content of nitrogen as any; further, the growth rate of these plants is actually higher (G.R.F. Report, 1931-32, fig. 14), so that as much total nitrogen must have been taken up. It seems that the second hypothesis is untenable, and hence also the first.

The reason for the failure must be internal to the plant; the proximal cause is the failure to produce flowers. This may be in part due to the high temperatures prevailing at the time immediately after sowing (temperature maximum in October), or to the sudden change in climatic conditions in November with rapid fall in humidity. The leaves of these plants are in fact smaller and thicker than of the earlier sowing, and they retain their nitrogen (Table V). Associated with this effect is the failure to produce lateral branches. Which is cause, which effect? A possibility of changed nitrogen metabolism has already been mooted and may be worth investigation. It is possible that the failure to produce lateral branches arises from a failure in the translocation mechanism of these plants, or, alternatively, an inactivation or absence of proteolytic enzymes leading to longer life of individual leaves and reduction in hydrolytic products available at the meristematic regions.

DR. GREGORY, in introducing his paper said he had raised what he considered to be the main problem of cotton culture in the Sudan in a very acute form. The problem of major economic importance was the annual fluctuation in the yields of cotton, which had been going on ever since cotton had been grown there.

The data which he had used were contained in the Reports of the Gezira Research Farm. Since the paper was written, he had also had the advantage of seeing some of the results of later experiments in the Sudan. When he arrived in the Sudan it was generally believed that the problem was one mainly of water supply. When he left, and since, the emphasis had fallen rather on to the questions of nitrogen supply to the plants and the effect of the nitrogen supply on the morphological development of the plant, and thence indirectly on the yield. He had heard rumours also that attention was now swinging away from the nitrogen position, and this paper might possibly remain as a relic of an exploded theory. However, before finally giving up his own views on this subject, namely, that nitrogen was of major importance, he would recommend his remarks for perusal, to see to what extent he could drive his own point of view to its logical conclusion.

It would be seen from the paper that his method of approach to the question of the annual fluctuations of yield was that he had contented himself with adopting a theoretical attitude towards the problem. He was out of touch completely with the experimental work in the Sudan, and was there only for six months in 1928-29. Therefore he regarded the problem from a distance, which was often to the good, since in those circumstances some of the salient features might appear which, if one regarded the problem from too close a point of view, one might be too distracted to see.

It would be seen that he very summarily dismissed the effects of the incidence of diseases and pests. He took that from the Reports which had appeared from the Gezira Research Farm. Therefore if in fact he had minimised the importance of that factor, the blame was not altogether his.

The possibilities of direct climatic effects were then discussed in the paper: primarily the effect of rainfall. It would be seen that in two particular years, which are discussed at some length—the years 1929 and 1930—the remarks about the climatic conditions would be somewhat misleading if one were to take them as a basis of a forecast of the crop. He had confined his attention mainly to experiments in those two years, because they were the occasions of two very elaborate exploratory experiments in which an attempt was made to evaluate the effect of all those factors (other than climate, of course) which might affect directly the yield of cotton. The results of those experiments, as far as the yield data were concerned, had already been published.



The morphological aspects of those experiments were not yet published, but a paper by Mr. F. Crowther on that subject would shortly appear.

The effect of rainfall was dealt with under various heads. Firstly, as to its possible direct effect, in so far as it leads to water-logging of the soil. He had quoted results of experiments which had in fact been done in the Sudan to investigate that point of view, and it would appear from them that one could not attribute the direct fluctuation of yield in the Sudan to variation in total rainfall. The experiments in which water-logging was deliberately carried out had shown that periods of seven days water-logging had had no detrimental effect on the plants so far as yield was concerned. Direct effects on the plants were immediately noted, such as the yellowing of the leaves, which, however, was not associated with a complete cessation of carbohydrate assimilation by the plants. Further, the nitrogen content of the leaves very rapidly fell under the conditions of water-logging. But the important point was that, after the water was removed, good yields were still obtained. From the paper it would be seen that in the two years mentioned the average yields from the complicated inter-action experiments were very closely in conformity with those obtained from those of the Gezira area, which indicated that the conditions over the whole Gezira were at least as variable as those imposed in the controlled experiments.

All the evidence of the yield data from different parts of the Gezira showed that within a single year there were very large fluctuations indeed in yield, which again showed that it would be difficult to attribute the small yield of a particular year to any one particular factor.

Continuing this line of destructive argumentation, Dr. Gregory said the effect of the date of sowing on the yield as compared in the two years was shown in Table 2. The important point there was that deliberate variation in sowing date was undertaken in those experiments, varying from July 22nd to September 23rd, with and without the addition of nitrogen. In the two years the relative yields where nitrogen was added were the same. If any one particular factor of the climate had been concerned—for instance, if it had been rainfall subsequent to, or at the time of, sowing, or if it were some change in humidity that occurred in the Sudan at variable times at the beginning of November—the plants in any one season would have experienced the same changes at different stages in their life histories, and the changes experienced were different in the two years. Obviously those plants which had been sown in September

could not have been influenced directly by the rainfall in July, and so on. Nevertheless, the relative yields in those two years for the different sowing dates were the same, which could only mean either that there must be some factor which affected all the sowing dates equally (*i.e.*, it must have been something antecedent to the first sowing date), or else it could not have been any direct effect of the climate at all.

To sum up this side of the question, there was one effect which Dr. Crowther pointed out in 1925, namely, that low yields in the Sudan are associated with rainfall there early in the season, in May and June. That, of course, fulfilled the requirements of the particular analysis under discussion, for there would seem good reason to suppose that if a climatic factor were concerned, it must be one such as this which must antedate all the sowing dates in the experiment itself.

Dr. Gregory said he believed that this early rain was responsible for some of the variations in annual yield. It would seem that the variations from year to year are associated with some change within the plant itself, and the evidence was fairly strong that it had to do with the nitrogen supply to the plant. Sowing date experiments which were done in the Sudan showed up one very important result, namely, that if cotton is sown late, then, in general, the yield falls, and this fall in yield is the more noticeable (that is, relatively) in plants which have received nitrogen rather than in those which have received no nitrogen.

Dr. Gregory said that the problem of late sown cotton was of very great importance. When he was in the Sudan, the first experiment carried out there on this point was done in two places, one in the Gezira and the other at Shambat. The experiment at Shambat with cotton sown at the normal time showed all the features which late-sown cotton in the Gezira normally showed. The failure of the crop late-sown in the Sudan was not due to a failure in the general growth, which seemed to indicate that it was not directly a problem of destruction of roots, or anything of that kind. The reason why the crop failed was that there were very few bolls on those plants. The plant was characterised by having a very rapid vegetative growth of the main stem and very little lateral growth of the sympodia.

Mr. Bailey years ago, working at Shambat, had already shown conclusively that this habit of growing tall without branching was a characteristic of cotton plants grown at high temperatures. The sowing date experiment—sowing plants from July to September—was indirectly an experiment on the effect of temperature differences on cotton plants.

The most interesting thing about the nitrogen data in the Sudan was that the nitrogen actually within the leaves of the plants at various sowing dates reached a maximum early in the life history of the plant. This absolute maximum of nitrogen which the plants contain depended entirely on the time of the year; in other words, the temperature conditions (if it were temperature) seemed to impose a maximum amount of nitrogen which the plant could contain at any particular time of the year. Evidence in the Sudan was very strong that the nitrogen content of the leaf directly regulated the leaf growth, and, further, there was incontrovertible evidence that the leaf growth of the plant determined to a very large extent indeed the yield of the plant. Plants sown late in September never contained as much nitrogen at their maximum as plants grown earlier in the year. Late-sown plants, therefore, were characterised by having many leaves, but with rather low nitrogen content. The reason why they had long stems and many leaves was because they do not have many bolls, and the cessation of vegetative growth in cotton in the Sudan was occasioned by the development of the bolls. Cessation of vegetative growth was normally secondary in the Sudan. In the late-sown plants, therefore, characterised by very leafy stems and few flowers, the nitrogen content never was very high; but on the other hand, the nitrogen content of those leaves did not fall off. Normally leaves on the cotton plant start off by having a high nitrogen content, and that very soon falls. That is true also of the whole plant. The question arises, where has this nitrogen gone? The answer is that the proteins of the leaf have been hydrolysed and the products of the protein hydrolysis have been used for further growth—either vegetative growth or the growth of bolls.

Dr. Gregory therefore asked whether the variation of the yield with sowing date might be due largely to the factors relating to the hydrolysis of the proteins in the leaf: that is, the death of the leaf did not take place in those circumstances: did that result in the fact that very little available free nitrogen was present within the plant for the production of bolls and of flowers and sympodia? He thought that must be considered as a possibility.

As regards the annual fluctuations in the yield, Dr. Gregory thought the evidence was very strong that the fluctuation was in fact associated with variations in the nitrogen supply to the plant. The immediate cause of this variation in nitrogen supply was admittedly not very clear. At one time, Mr. Crowther Junr., Mr. Crowther Senr. and he were under the impression that possibly what happened with late application of ammonium sulphate was that for some reason or other, the ammonium sulphate either became unavailable or was removed.

The evidence seemed to be against this: in Table 5 of the paper it would be noted that, with the different sowing dates, the average content of nitrogen of all the leaves remained constant. Therefore, since the late-sown cotton had, in fact, at least as many leaves as the early sown cotton, the drop in yield which such cotton showed, could not be due to the fact that the plant did not receive that nitrogen. It must be due to the fact that the plant utilised the nitrogen in a different way. In other words, that with late-sown cotton, the nitrogen was locked up in the leaves in some way or other, and did not become available for boll formation.

He suggested that some similar factor might be important in determining the variation of yield from year to year, second only to the direct importance of the early May-June rainfall, whose effects on the soil were a matter for study by soil chemists. It was known that years in which the early rainfall was heavy, were characterised by very low vegetative growth of the cotton plant. Presumably, therefore, the plant did not receive the full modicum of nitrogen from the soil, and in fact, some change had taken place in the soil preventing the plant in some way from obtaining the nitrogen which was there.

#### DISCUSSION.

MR. BAILEY, in commenting on Dr. Gregory's paper, said that it was unfortunate that Dr. Gregory should have chosen the two seasons 1929-30 and 1930-31 to illustrate the general problem of the fluctuations in yield in the Gezira, as both those years happened to be years of very low yield, compared with the average, and it might reasonably be assumed that the same major depressant factor was operating in both cases. The yield in 1930-31 was actually the lowest on record.

He suggested that it might well be that the difference in yield between these two seasons might be explained by differences in nitrogen supply, but that was not to say that nitrogen supply was the dominant controlling factor in the Gezira. It was probable that some of the difference between the two seasons which Dr. Gregory had considered was, actually, due to Leaf Curl, which reached its worst in 1930-31.

Whilst it was a fact that the supply of nitrogen was a matter of very great importance in the case of early-sown cotton, it had been found that the late-sown cotton had an internal nitrogen level which was consistently above the minimum during the two seasons under discussion. Further, the level of nitrates in the soil rises rapidly during August to November: all indications are, therefore, to the effect that with September sown cotton, nitrogen supply is not the main factor limiting the growth of the crop.



Yet reference to Table II in Dr. Gregory's paper showed that the yield of September-sown cotton in 1930-31 was reduced to precisely the same degree—compared with 1929-30—as the yield of the August sowings. Did not this suggest that some other factor was at work?

Dr. Gregory had noted that 1930-31 was a year of high rainfall in May and June, but 1929-30 was equally high in this respect, and in that year also was recorded the highest total seasonal rainfall at the Gezira Research Farm. It was likely that the effects of that exceptional rainfall would carry over, in part, to 1930-31 and intensify the effect of the early rainfall in the second year.

Dr. E. M. Crowther's discovery in 1925 that heavy rainfall in May and June was associated with low yields had received further confirmation as time went on, and during the season just past, Mr. F. Crowther, in collaboration with Mr. Snow, had demonstrated experimentally at the Gezira Research Farm that total exclusion of early rain (by transparent screens) led to a significant increase, in yield per plant, of 24 per cent. over plants growing in soil which had received normal exposure during that period. The early growth of the plants in both series showed no obvious difference to the eye. Mr. Crowther found that the plotlets from which early rain had been excluded contained less nitrates, down to a depth of 4 feet, than those on which early rain had fallen.

Other observations all suggested that early wetting of the soil leads to early nitrification, resulting in higher values in August, when the crop is sown—yet early rains are apparently followed by lowered yields.

Mr. Bailey went on to observe that, in the two seasons of poor yield just past, the growth of the plants was excellent up to a point, sometime in October, when further growth was restricted and heavy shedding set in; he gave it as his opinion that this was probably characteristic of all seasons of poor yield, where the early growth is not crippled by Blackarm or surface flooding.

The workers at the Gezira Research Farm had suggested two ways in which early rain might interfere with the extensive depth-cracking, which was such a marked feature of Gezira soils—the high humidity would tend to limit the extent of this cracking, and the rain would tend to seal the cracks from below. Plants sown in such soil would be expected to grow satisfactorily until their roots reached that lower, increasingly anaerobic layer, when their growth would presumably be affected, either directly or through the conditions becoming more favourable to the growth of certain fungi, which would be referred to by Mr. Massey.

It was not unlikely that the ultimate explanation of the wide yield fluctuations in the Gezira lay along those lines.



## PHYSIOLOGICAL PROBLEMS OF THE COTTON PLANT.

*Mr. Bailey pointed out that Dr. Gregory had referred to the reduction in area of leaves which was associated with late-sown cotton, and stated that Mr. F. Crowther had recently made a most interesting series of observations on that point, which indicated the cause of diminishing returns with late sowings.*

*He suggested that if "climate" could produce effects of that magnitude within a single season, it was impossible to rule out the possibility of comparatively minor "climatic" differences between seasons having a considerable effect on yield.*

*DR. E. M. CROWTHER said that, as reference had been made to his discovery in 1925 of a correlation between early rains and cotton yields in the Gezira, he might add the results of a recent re-examination of the whole of the data available. The bad effects of early rains were fully confirmed but, in addition, it was found that the total rainfall, and, at Tayiba, the previous year's rainfall, were also negatively correlated with yields. The first hypothesis proposed to account for the striking adverse effect of early rains was that the first wetting of the soil initiated microbiological activity, including nitrification, and that in some way this reduced the amount of nitrogen subsequently available to the plant at a critical period. It was admitted, however, that subsequent work had not substantiated this hypothesis. Until quite recently work on the soil nitrate and ammonia had been restricted to the surface foot of soil, but Mr. Bailey had just stated that the last year's work had revealed effects of the early rain on the distribution of nitrate and water throughout the soil profile. It appeared that the early rains closed up cracks in the deep subsoil and restricted the water supply to the plant at a critical stage in late autumn.*

*Dr. Gregory's hypothesis that late-sown cotton suffered from an inefficient utilisation of nitrogen at high temperatures obviously deserved systematic study. Recent field experiments had demonstrated the importance of very early application of nitrogenous fertilisers. In no experiment yet made had an early application given a poorer result than a later application. It might be that one of the troubles with late-sown cotton lay in the difficulty in getting nitrate down to the depth of the soil where the roots are active during late autumn.*

*The most interesting general feature of the experiments Dr. Gregory had described was the demonstration of the value of a systematic study of the cotton crop by complex experiments testing combinations of different types of treatment. The inter-actions revealed in such experiments might be of the utmost value in suggesting the mechanism of the effects. It was of enormously greater importance to know how the effect of added nitrogen depended on sowing date, spacing, etc., than to know merely that under practical conditions added nitrogen sometimes gave an effect and sometimes did not.*

MR. SAWHNEY referred to the paragraph in Mr. Jackson's paper which dealt with Leaf Roll. It had been stated in the discussions earlier in the day that that phenomenon was chiefly due to physiological conditions. In Allahabad, which was only a few hours from Indore, the same symptoms had been found to be associated with an intense jassid attack on the crop. In order to confirm the responsibility of the jassids, an experiment was carried out in which a few plants were grown under a covering of thin muslin. These grew normally, and showed no sign of disease. When the plants were again exposed to jassid, the characteristic symptoms of leaf curl appeared.

MR. N. W. BARRITT, speaking on Mr. Jackson's paper, suggested that there was a difficulty in attributing to algae the responsibility for the reduction of nitrates in humid cotton soils. Both the reduction and assimilation of nitrates involved the utilisation of a corresponding amount of carbohydrate or other energy material. In this respect algae could only function in the presence of light and since they possessed no roots they could only utilise the nitrates on the surface layer of the soil, which under humid conditions would tend to be leached out.

With regard to the effect of compost, he asked how much was required to produce optimum yield. Under tropical conditions organic manures appeared to be even more important than in temperate climates. For example, in Northern Nigeria, Greenwood and Hartley had found that an optimum effect was obtained with a small application of 2 tons per acre of dung, and similar results had not been obtained with any application of artificial fertilisers.

The action of a surface layer of heated soil in increasing the yield of a deep rooted crop like cotton, recorded in the paper, was remarkable and might be due to increased availability of N.P.K. rather than to an improved soil texture. More chemical analyses would be required to determine the cause.

With regard to Dr. Gregory's paper, he thought a good case for the nitrogen factor had been made out. In discussing the increase in nitrates immediately following rain, it should be remembered that nitrate was the most mobile form of nitrogen and hence the most easily lost, if not immediately taken up by the plant. An increase in nitrates in the early part of the year was not necessarily available to the crop sown in the autumn and might even signify a reduction in total nitrogen available to the subsequent crop, and thus confirm the theory put forward by Dr. Crowther and Dr. Gregory.

MR. MASSEY said that the description of root rots in India given by Mr. Jackson was of particular interest to the Sudan at the present time, as they had been investigating similar troubles in the Gezira during the past winter.

*A wilt of cotton, associated with a Fusarium of low pathogenicity had been known in the Gezira for at least ten years and had been associated by him with a water-logged subsoil. A more intensive attack last November led to more thorough investigations, when it was found that loss of the fine rootlets was not confined to wilting plants, but was general. Naturally it varied considerably in degree, but was sufficient to explain the shedding that had taken place during the previous 6-8 weeks, and the shrinking of the crop generally.*

*The rotting was accompanied by fungi, of which some five species had been isolated, all of which were known to be parasitic. In addition, a phycomycetoid endophyte with vesicles and arbuscles was found to be commonly present in the finest rootlets.*

*Mr. Massey believed that the trouble was associated with the water content of the soil and the nature of the soil, which was a heavy alkaline clay. Investigations were being continued with special reference to the water duty and the aeration of the Gezira soils.*

DR. BALLS said he thought Mr. Massey's communication was most important. There was a definite turning point for the Sudan-Gezira problem in what he had indicated, and in the right direction.

MR. JACKSON, in replying to the discussion on his paper, said that, with regard to Mr. Sawhney's statement regarding the relation of jassids to leaf roll, he suggested a possible explanation of the phenomenon he described, which agreed with the results of experiments at Indore in the matter. During the presence of the muslin protection in Mr. Sawhney's experiments, no rain-splash could materially affect the soil, and it had been found at Indore that rain-splash—the puddling of the surface layer of the soil—would produce leaf roll.

MR. SAWHNEY interposed a remark that the crop was grown after the rains, and under irrigation.

MR. JACKSON said that of course in that case his suggestion supplied no explanation at all, but it did not alter the fact that a similar phenomenon could be produced by puddling; the plant could be checked, and growth again produced by removing that puddled condition.

*He welcomed Mr. Barritt's remarks on algae; on black soils during the rain algae certainly grew very vigorously, and made a very thick gelatinous layer.*

*With regard to the effect of a 6 in. layer of heat-treated soil upon the resultant crop, a fact that was not mentioned in the paper was that a number of thicknesses of layer had been tried, and a very noticeable effect was produced by as thin a layer as  $\frac{1}{2}$  in. simply spread on the*

surface and not worked in. It seemed to be that the protection of the surface was responsible for the effect.

Mr. Jackson said he would bring Mr. Barritt's suggestion as to the greater availability of minerals in that burnt soil to the notice of his chemical colleagues. At the same time he did not think it was likely to be the cause of the effect observed, because no surface dressing of manure produced similar results.

With regard to the application of compost, in the experiment described, speaking from memory Mr. Jackson thought it would be between 10 and 20 tons to the acre. The point Mr. Barritt had raised as to small doses was one which was being investigated in the current year. Some work recently carried out in the last few years in one of the dry States of America brought out the fact that too large a dose of a humic manure actually had a depressing effect on yield, or, at any rate, it had nothing like so marked an effect as a very much smaller dose.

With regard to root washings on wilted and non-wilted plants, it was true that with the technique at their disposal at Indore they had found it quite impossible to prevent the destruction or washing away and loss of extremely fine rootlets. At the same time it was possible to discriminate between those three classes of rootlets which he had illustrated in the slides—the really dead, the dormant or non-absorbing roots, and the new, active, white tissue which was obviously absorbing rapidly. Furthermore, seeing that all exposures were carried out under the same conditions, the same proportion of loss of fine rootlets would take place whether the plants were wilted or not wilted.

While agreeing with Mr. Massey that the attacks of the fungi upon the roots of the plant might be brought about by soil condition, he mentioned that the work at Indore had not yet reached a stage at which it was possible to be sure on this point.

DR. GREGORY, in replying to the discussion on his paper, said he felt somewhat chastened after the remarks that had been made. He considered it by no means a disadvantage to have used the two years in which the yield was so bad, because the responses to particular treatments in those two years were extremely large. If Mr. Bailey wished to put aside all important considerations on the results drawn from those two years' experimentations, he would ask him also to put aside all the conclusions he himself had drawn from those data about the inter-action of factors. Those were the two years in which those four factor experiments were carried out.

He admitted that he was in complete agreement with what Dr. Crowther had said. He (Dr. Gregory) had intentionally rather ridden his hypothesis to death, and he had perhaps been treated as he deserved.

## THIRD DAY.

### FIBRE PROPERTIES OF COTTON.

*Chairman* : Dr. R. H. PICKARD, F.R.S.

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#### STUDIES IN VARIABILITY OF COTTON, WITH SPECIAL REFERENCE TO IMMATURITY.

BY

F. T. PEIRCE AND E. LORD (*Shirley Institute*)

#### PART I.—TEST RESULTS, PROCEDURE AND SAMPLES.

Variations of test results, of population values and of characters within samples affect the choice of cottons from seed selection to matching of cloth, the adaptation of machinery, and the behaviour of the cotton in process.

The practical and observed variability arises from two fundamental variations in biological origin, of strain and of culture. The strain variations may be of recognised differences of variety, of mixture of varieties after ginning, of seed mixture or of variable genetic character in a supposed uniform strain. (The last two may react with and be manifested by cultural differences.) The cultural differences may arise from transference of a variety to a new growing country, differences of climate, irrigation and soil within a country, seasonal variations, growth differences between plants in one field and variations in nutrition of hairs on the same plant and seed.

The many different kinds of variation are observed and shown only by the variations within and between tests, particularly in the combined stapling test. They must be evaluated by analysis of the test results on samples of known origin. The data are incomplete and can be supplemented by co-operation with agricultural workers. In the present paper, the available material is analysed to show the magnitude of the variations which affect the interpretation of results, the relative importance of the various sources of variation, and to differentiate between strain and cultural variations. While the paper cannot claim any finality it may indicate and suggest lines and methods for further studies.

*Bale Variations.*—Surveys have been made of the variations of length and hair-weight obtained in stapling tests on samples taken from 25 bales of one mark and delivery of Texas cotton. From each bale, 5 samples were taken, a handful (a broker's sample), and



4 "Shirley" samples, from the two sides of a top and a middle layer. Significant variation was found between bales, but not within bales. Thus, no evidence was found of "false packing," and the crude "broker's sample" gave results not significantly less representative than the careful random sampling of many small lumps. A similar result was obtained in a survey of 10 Californian bales. (See Appendix A).

The conclusion could not be safely drawn that variations within bales are negligibly small, but must be that the error of a test result as the description of a bulk sample is large.

*Duplicate Tests.*—The large size of this error has long been recognised and has been often investigated in the development of the test procedure. From the bulk sample, many small tufts are taken to make up an "envelope" sample. These tufts are doubled, drawn and halved, till the sample is reduced to about the size of one of the original tufts, some 15 mgm., which is the test sample, all of which is included in the diagram. The number of tufts has been successively increased as the need for higher accuracy was recognised and as trials showed that increase in number did increase accuracy. At present, 64 tufts are taken and the laborious sampling and handling involved is excessive for routine purposes.

An analysis has been made of the difference between duplicate tests by the present procedure, to evaluate the magnitude of the errors and to detect any difference between observers. The examples were divided into 5 groups. (a) 98 Sakel type cottons. (b) 24 Uppers type. (c) 180 exotic Americans, all experimental growths. (d) 117 miscellaneous commercial cottons. (e) 67 miscellaneous yarns. 32 examples were also included in the analysis, of duplicate tests made on samples of 32 instead of 64 tufts. These last data showed that the accuracy was considerably improved by doubling the number of tufts.

The Standard Error of the value of a character from a single test was found to be as follows (expressed as per cent. of the value):—

TABLE I.

|                                     | Effective Length. | Hair-weight per cm. |
|-------------------------------------|-------------------|---------------------|
| Sakel type ... ..                   | 2.7               | 3.8                 |
| Uppers type ... ..                  | 2.7               | 5.2                 |
| Exotic American ... ..              | 3.7               | 4.8                 |
| Commercial cottons ... ..           | 3.7               | 2.4                 |
| Commercial yarns (many dyed) ... .. | 4.0               | 4.3                 |

## STUDIES IN VARIABILITY OF COTTON.

For a difference between single tests to be significant, it should exceed three times these amounts; a difference between the means of two duplicate tests should exceed twice the Standard Error. The results of immaturity tests are not in a suitable form for such an analysis, and they are being analysed in the form of "maturity ratios" described below.

The significant differences are uncomfortably large for safety in many uses of the test, but it would be quite impracticable to increase the number of tufts to lower the margin of uncertainty. Nor can the variation found in duplicate tests be ascribed mainly to any insufficiency in the number of independent tuft samples, but rather to the imperfection of the mixing. The procedure of mixing is devised and carried out in a most careful and extremely laborious manner. The observers are well trained and periodically checked, and analysis of the data indicates that observer differences are an insignificant factor in the variation. (Appendix B.)

*Tuft Variations.*—The error of stapling tests arises mainly from the great variability of character between tufts of hairs as they lie in the ginned cotton, and the mechanical difficulty of dissociating these tufts into single hairs and taking a truly random selection of them, uninfluenced by the original association, length, rigidity, clinging power, etc. The variation between tufts, which gives some indication of variation from seed to seed, was investigated by making diagrams of ten tufts from each of five varieties. Each tuft was of 20 mgm. approximately and was taken just as it lay in the bulk. From the variance between the results on each group of ten tufts, the Standard Deviations were calculated and are given in Table II.

TABLE II.

| <i>Variety.</i>                        | <i>Sakel.</i> | <i>Mississippi.</i> | <i>Texas.</i> | <i>Tanganyika.</i> | <i>Westerns.</i> |
|----------------------------------------|---------------|---------------------|---------------|--------------------|------------------|
| Effective Length in $\frac{1}{16}$ in. | 47            | 39                  | 32            | 33                 | 26               |
| S.D. ... ..                            | 2.3           | 1.7                 | 1.7           | 3.5                | 4.1              |
| % Short Fibre ... ..                   | 13            | 16                  | 20            | 17                 | 26               |
| S. D. ... ..                           | 6.0           | 5.9                 | 5.8           | 8.8                | 8.0              |
| Hair-weight ... ..                     | 145           | 180                 | 223           | 149                | 227              |
| S. D. ... ..                           | 13.2          | 14.4                | 21.3          | 46.5               | 27.6             |
| Range ... ..                           | 125-165       | 157-198             | 187-251       | 89-214             | 178-273          |

A desirable level of accuracy would be attained if the S.D. of a test were 1 per cent. of the length, 1.5 per cent. of the hair-weight. The S.D. of length for one tuft of Sakel is 2.3, or 4.9 per cent., so the

desired accuracy should be obtained by the perfect mixing of 24 independent tufts. It is not ; nor by 64 tufts. Similar calculations for the various characters and varieties give similar results, and indicate that the 64-tuft sample should be much more accurate than it actually proves to be.

An efficient mechanical mixing of a bulk of cotton dissociated into individual hairs is the desideratum for reproducible results. This is actually achieved in processing for spinning, in particular in the finisher sliver of a spinning test. Stapling tests have shown that there is a negligible difference between the characters of carded sliver and bale cotton, and the former seems to be the best material for stapling tests. It is more useful to test the sliver than the roving, as the results can thus be obtained before any process demanding a close adjustment of roller settings to staple length.

## PART II.—DIFFERENCES OF STRAIN AND CULTURE—STANDARD HAIR-WEIGHT AND MATURITY RATIO.

Putting aside the problem of errors of observation, consideration will now be given to the real variations, their effects and practical importance. The variations are to be found between the hairs which comprise any bulk of cotton, the differences between the distribution of character in different lots of nominally similar cotton, and the differences between varieties.

*Character Variation within Single Lots.*—The great variation of character among the hairs in any sample of cotton is the source of the difficulty of reproducible tests, which has just been discussed. It is also an essential feature in the behaviour of cotton as a textile material, and much study has been, and has to be, done, to determine the effects of the distribution of length, hair-weight and immaturity on spinning and on cloth quality and wear. Variability of character enters so frequently into such a range of technical problems that its control may confidently be claimed as a major question of scientific technology, even though the precise effects of character variations have still to be evaluated. Generally speaking, it is clear that a practical limitation lies in our inability to attain sufficient uniformity for the best technical results.

The heterogeneity of character may be exaggerated by the mixture of varieties either of cotton after ginning, or of seed, or by genetic impurity of seed. To some extent, commercial malpractice or carelessness conduce to such mixture and that aspect of the problem is administrative. But in the purest commercial or even experimental

strain, there remains great variation of character among the hairs. The degree of uniformity is a feature which appeals to the commercial user, and the technical scientist would be interested to know how a highly uniform cotton would behave. It is therefore a feature which might well be considered in selection work and made the subject of investigation in pure strains.

*Factors of Variation in Growth.*—Only in the field is it possible to obtain the material to determine the sources of variation among hairs in a pure strain and their qualitative and quantitative effects. What conditions affect the length, cell diameter and wall thickening respectively? How do these vary over a single plant and seed, and during the season? Could no simple field criteria be discovered to enable special pickings to be made of particularly uniform material? Studies have of course been made on these matters, but only the very crudest outline of an answer can yet be seen.

In considering variation from the growth point of view, it is essential to consider it in terms of quantities directly related to the growth factors. These are, in order of time at which they are determined, cell diameter, length, and degree of thickening; and these quantities are determined by a reaction between genetic character and environment. In the rough answer available to the problems of the previous paragraph, one feature appears to be that degree of thickening is mainly determined by cultural factors; length and diameter are more specific. Probably the amount of short fibre is more affected by culture than the length of the longer hairs, so that the effective length should be a more constant feature of the strain than the mean length. Cell diameter seems highly specific, as to mean value, but it is highly variable even in pure strains and is probably influenced by internal environment—position on seed, etc. Perhaps some feature of the distribution, or some class of hair—such as normal hairs of the effective length—might show more uniform and highly specific values than the general distribution as a whole.

Field studies only can develop to their full precision any such criteria to distinguish between strain and cultural variations; but a gross imperfection is evident in the immediate results of the stapling tests when they are considered in relation to the botanical factors. The hair-weight is equally dependent on the independent factors of cell diameter and degree of thickening. As, however, the test supplies data, in the form of the immaturity count, on the degree of thickening, it should be possible to re-express the two test results—hair-weight and immaturity—by two quantities which separate the specific fineness and the environmental degree of maturity.

*Relation between Hair-weight and Immaturity.*—It is evident, in following the results on a constant strain grown in different places and seasons, that the effective length is relatively constant and that large variations of hair-weight are inversely correlated with high degrees of immaturity. From the latter correlation, it should therefore be possible to evaluate a correction to the hair-weight as a function of immaturity, to obtain a hair-weight corresponding to a standard immaturity figure and relatively constant for a uniform strain.

To obtain the necessary function, it is convenient to combine the three percentages of the immaturity count into one quantity. At the first attempt, not knowing the relative hair-weights of the three types, an arbitrary quantity was used, immaturity  $I = D(N+D)$ , where  $D$  is the percentage of dead,  $N$  of normal hairs. This, in effect, divides the intermediate class of "thin-walled" between the two extreme groups in proportion to their own number. The best group of constant strains available were 30 samples of Sakel type, which were divided into four more uniform sub-groups. The quantity  $1/H$  was correlated with  $I$  by a coefficient  $+ 0.75$  and a regression formula was found which could be put in the convenient approximate form  $H_1 = H(0.9 + I)$ , where  $H_1$  is the hair-weight at a standard immaturity condition,  $I = 0.10$ . This quantity,  $H_1$ , showed only half the variance of the actual hair-weight  $H$  within the sub-groups of similar strain; and there may have been still some real genetic variation within these groups.

After a period, when 60 samples were available, the correlation obtained had fallen to 0.48 and a correspondingly flatter regression was obtained. Possibly more genetic variation had occurred through the years, and such correlations should be made on groups with larger numbers of samples of accurately controlled strain purity grown under widely different conditions.

*Weight of Normal, Thin-walled and Dead Hairs.*—After considerable labour and care, it was, however, found possible to attack this problem more directly by physically separating the normal, thin-walled, and dead hairs, and determining their separate hair-weights per centimetre. Bunches of hairs were first cut to a centimetre length, immersed in 18 per cent. sodium hydroxide solution, and examined microscopically. The hairs were then separated into the three familiar groups of the immaturity test, and the three bundles of hairs resulting were separately washed, dried, weighed and counted. Some 500-800 hairs were obtained from each cotton, from 5 points down the comb diagram, as for sorted hair-weights. The results on 7 samples are shown in Table III.



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TABLE III.

| Type.            | Hair-weight per cm. of |              |       | Ratio to Normal |       | Immaturity. |
|------------------|------------------------|--------------|-------|-----------------|-------|-------------|
|                  | Normal.                | Thin-walled. | Dead. | Thin-walled.    | Dead. | N—D.        |
| Sakel (a) ... .. | 165                    | 120          | 71    | 0.73            | 0.43  | 64-9        |
| Sakel (b) ... .. | 169                    | 130          | 67    | 0.77            | 0.40  | 67-12       |
| Mexican ... ..   | 238                    | 179          | 114   | 0.75            | 0.48  | 59-11       |
| Texas ... ..     | 267                    | 200          | 135   | 0.75            | 0.51  | 57-15       |
| Surtee ... ..    | 280                    | 214          | 123   | 0.78            | 0.44  | 59-19       |
| Ecuador ... ..   | 301                    | 229          | 159   | 0.76            | 0.53  | 54-16       |
| Broach ... ..    | 327                    | 220          | 110   | 0.68            | 0.34  | 56-9        |
|                  | Mean ratios ...        |              |       | 0.75            | 0.45  | —           |

The immaturity count is really a frequency distribution with three intervals and each interval must cover a rather wide variation of thickening. Real variations in the relative mean hair-weights of the groups must be possible and the ratios may vary in correlation with immaturity, but, empirically, the ratios appear to be practically constant over the range of most practical interest, covered by the 7 examples. More studies are being made on the value of the ratio, but it seems clear that general mean values, equal to those in the table, may be assumed for many purposes.

*Standard Hair-weight and Maturity Ratio.*—If, in 100 hairs of a sample, there be N normal hairs, T thin-walled and D dead, as shown by the immaturity count, and if  $H_n$  be the hair-weight of a normal hair, the average hair-weight of the sample (H), as obtained from the usual test, will be  $H_n (N + 0.75 T + 0.45 D) / 100$ , and the “normal-hair-weight” may be calculated from the stapling test results by the formula :

$$H_n = \frac{100 H}{N + 0.75 T + 0.45 D}$$

It would be expected, and it is found, that the “normal-hair-weight,”  $H_n$ , is a much more stable specific character, independent of growing conditions and immaturity, than is H. The factor,  $M^1 = (N + 0.75 T + 0.45 D) / 100$ , will be called the “Maturity Coefficient.” It serves to express the result of the immaturity count as a single quantity, suited to quantitative analysis and relatively independent of the arbitrary features of the test. It is simply a ratio of hair-weights, of the average to the normal. Barring any small correlation of diameter with immaturity, it is similarly a measure of the ratio of areas of cross-section, and a function of the ratio of wall-thicknesses. The configuration of a “normal” hair remains an arbitrary feature of the microscopic test, but this is easily eliminated.

The “normal-hair-weight” is not the hair-weight of a normal sample of a variety, but of a sample containing 100 per cent. normal hairs, which would be quite abnormal. It is therefore not very

suitable as a measure of the hair-weight of a cotton, corrected for immaturity. A better value would be the hair-weight of a normal sample of well matured cotton. The immaturity count on such a cotton may be taken from general experience as 64·7, and this will be taken as the standard of comparison. The corresponding maturity coefficient is  $(64 + 0\cdot75 \times 29 + 0\cdot45 \times 7) / 100 = 0\cdot889$ . Then, the ratio of the hair-weight of a sample to the hair-weight of standard maturity may be called the "maturity ratio,"  $M = (N + 0\cdot75 T + 0\cdot45 D) / 88\cdot9$ , and the "standard hair-weight,"  $H_s = H/M$  ( $= 8H_n/9$ ).

By making the "standard hair-weight" the basis of comparison, the corrected test figures are brought into closer relation with familiar experience; but the further advantage is obtained that the arbitrary features of the immaturity test are eliminated, save as means to an end. The maturity ratio is a measure of the ratio of wall section in the sample to that in a normally thickened sample of the same strain. Any criteria or tests might be used to describe the normal and actual samples in terms of their wall sections, but the ratio should be unaffected, save in so far as the tests are imperfect for their supposed objects.

*Relation between Measures of Thickening.*—Before considering the application of this new method of analysis and expression to the data of cotton-growing experience, some attempt should be made to compare it with the other methods available of measuring cell diameter and wall thickness, and quantities dependent upon these. (Both the comparison and the application are still very incomplete, but an early discussion may facilitate co-operation with growers in further studies.)

Sections were cut of two samples of Sakel cotton, by a method designed to secure a minimum of distortion or obliquity, after separating the three groups of the immaturity test. The ratios of the areas of cross-section to those of the normal group were: Egyptian, Thin-walled 0·69; Dead, 0·38; Sudan, Thin-walled, 0·74. These values are in good agreement with the ratios of hair-weight. Observations were also made on the ratio of the shortest to the longest dimension of each section. Taking the former as a measure of twice the wall thickness, the latter as the ribbon-width, the ratios of wall-thickness to ribbon-width were:—

TABLE IV.

|                    | <i>Egyptian.</i> | <i>Sudan Sakel.</i> |
|--------------------|------------------|---------------------|
| Normal ... ..      | 0·41             | 0·41                |
| Thin-walled ... .. | 0·28             | 0·30                |
| Dead ... ..        | 0·15             | —                   |

These figures demonstrate the objectivity of the immaturity test as a measure of the degree of thickening. There is some overlap of the groups, which may be ascribed mainly to local variations of thickening, but the limits of the thin-walled class are defined roughly by the values 0·2–0·35 for the ratio of wall-thickness to ribbon-width. Measurements show further that there is no significant difference between the perimeters of the three classes.

Perhaps the most fundamental manner of expressing the degree of thickening is by the ratio of the wall-thickness to the radius of the circle with perimeter equal to that of the hair, which will be denoted by  $\theta$ . By combination of various data, too involved for detailing here, it is estimated that the thin-walled class roughly represents a range of  $\theta = 0\cdot33$  to  $0\cdot5$  in the raw, unswollen hairs; and that the average hair-weights of the three classes correspond to values of about 0·7, 0·44 and 0·24 respectively. The average hair-weight of a sample with the standard degree of immaturity, 64·7, is that of a hair with the configuration expressed by  $\theta = 0\cdot57$ .

The values may be also expressed as the ratio of the area of the wall section to that of a circle with the same perimeter; and this ratio is estimated to be 0·81 for the standard, 0·91 for the normal hairs. It may be taken to equal  $\theta(2-\theta)$ , and to be proportional to the Maturity Ratio. These relations provide an absolute, geometrical interpretation of the Maturity Ratio and Standard Hair-weight. They are put forward tentatively, pending further experiment and discussion. (As the above ratio of areas  $= 0\cdot81 M = \theta(2-\theta)$ ;  $\theta = 1 - \sqrt{1-0\cdot81 M}$ .)

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By definition, the hair-weight  $H = H_s M$ , where the standard hair-weight is proportional to the area of the circle having the perimeter of the hair, or to the square of the perimeter. It would be expected to remain constant for samples of similar strain grown under widely different conditions, if these affect only the secondary thickening and not the cell diameter. In the field, it is possible to obtain measurements of the uncollapsed cell diameter and a comparison with standard hair-weight from our later stapling tests would be valuable.

*Swollen Diameter.*—With raw cotton in the usual desiccated state, a good approximation to the uncollapsed cell diameter can be obtained by swelling in 18 per cent. sodium hydroxide solution. Table V gives the values obtained on 32 cottons of widely different degrees of maturity and fineness. Measurements were made on the normal hairs only, because of the varying geometrical form of the immature hairs. They were made in conjunction with the usual stapling test, on the same hairs used for the immaturity count and hair-weight determination, from which the Maturity Ratio and Standard Hair-weight were calculated.

The plot of the square of the swollen diameter,  $D$ , against Standard Hair-weight is shown in Fig. I. It is a close relation, with deviations fully explicable by experimental error, but it deviates appreciably from a straight line through the origin.

TABLE V.

| Type.                     | Hair-weight, Immaturity and Swollen Diameter<br>Determinations on 32 Cottons. |             |                       |                                      |                       |
|---------------------------|-------------------------------------------------------------------------------|-------------|-----------------------|--------------------------------------|-----------------------|
|                           | Hair-weight<br>per cm.                                                        | Immaturity. | Maturity.<br>Ratio M. | Standard.<br>Hair-weight.<br>$H_s$ . | Swollen.<br>Diameter. |
| Nahda ... ..              | 155                                                                           | 65—2        | 1.020                 | 152                                  | 21.78                 |
| F. B. ... ..              | 126                                                                           | 58—10       | 0.973                 | 129                                  | 20.15                 |
| Texas ... ..              | 226                                                                           | 55—5        | 0.981                 | 230                                  | 25.11                 |
| Sakha 7 ... ..            | 125                                                                           | 69—4        | 1.024                 | 122                                  | 20.31                 |
| Peruvian Pima ... ..      | 160                                                                           | 51—14       | 0.940                 | 170                                  | 22.56                 |
| Maarad ... ..             | 141                                                                           | 74—2        | 1.045                 | 135                                  | 21.13                 |
| Sakel Domains ... ..      | 133                                                                           | 62—8        | 1.008                 | 132                                  | 20.91                 |
| Ecuador ... ..            | 242                                                                           | 48—17       | 0.921                 | 263                                  | 27.08                 |
| Bowed ... ..              | 230                                                                           | 53—12       | 0.952                 | 242                                  | 25.17                 |
| Uppers ... ..             | 188                                                                           | 69—5        | 1.021                 | 184                                  | 22.62                 |
| Californian ... ..        | 194                                                                           | 58—9        | 0.976                 | 199                                  | 23.90                 |
| Iquitos ... ..            | 265                                                                           | 63—8        | 0.994                 | 267                                  | 27.68                 |
| Sudan American ... ..     | 159                                                                           | 46—11       | 0.936                 | 170                                  | 22.20                 |
| Nigerian ... ..           | 173                                                                           | 58—13       | 0.963                 | 180                                  | 22.50                 |
| Texas ... ..              | 207                                                                           | 55—15       | 0.948                 | 218                                  | 24.46                 |
| Sakel ... ..              | 135                                                                           | 64—7        | 1.000                 | 135                                  | 21.26                 |
| V. 135 ... ..             | 171                                                                           | 70—6        | 1.020                 | 168                                  | 22.28                 |
| Russian ... ..            | 245                                                                           | 68—2        | 1.028                 | 238                                  | 25.96                 |
| Sakha 7 ... ..            | 165                                                                           | 68—2        | 1.028                 | 161                                  | 21.98                 |
| Sakha 7 ... ..            | 151                                                                           | 72—1        | 1.043                 | 145                                  | 21.15                 |
| Broach ... ..             | 264                                                                           | 62—10       | 0.984                 | 268                                  | 28.54                 |
| Chinese ... ..            | 337                                                                           | 73—6        | 1.029                 | 328                                  | 32.16                 |
| Rangoon ... ..            | 287                                                                           | 81—5        | 1.055                 | 324                                  | 30.84                 |
| Gossypium Arboreum ... .. | 356                                                                           | 96—1        | 1.110                 | 321                                  | 30.94                 |
| Madras Northerns ... ..   | 213                                                                           | 58—7        | 0.983                 | 217                                  | 24.59                 |
| Oomras Improved ... ..    | 274                                                                           | 66—5        | 1.012                 | 271                                  | 27.58                 |
| Surat ... ..              | 303                                                                           | 76—4        | 1.044                 | 290                                  | 29.75                 |
| Chinese ... ..            | 412                                                                           | 82—2        | 1.067                 | 386                                  | 35.39                 |
| Karunganni ... ..         | 223                                                                           | 68—9        | 1.004                 | 222                                  | 25.69                 |
| Hagari ... ..             | 238                                                                           | 68—6        | 1.015                 | 234                                  | 25.96                 |
| Khandelsh ... ..          | 319                                                                           | 83—2        | 1.070                 | 298                                  | 30.26                 |
| Tinnivelly ... ..         | 232                                                                           | 68—8        | 1.008                 | 230                                  | 25.25                 |

Theoretically,

$$H_s = r^2 \cdot 0.81 \cdot \pi \rho,$$

where  $\rho$  is the density of cellulose in the raw hair. If the swollen perimeter is proportional to, say,  $\alpha$  times the raw perimeter,

$$H_s = 0.636 \cdot \frac{\rho}{\alpha^2} \cdot D^2$$

The deviation of the empirical relation from a straight line through the origin may be due to some variation of swelling or shape of swollen section, degree of thickening of normal hairs, density of cellulose or error of measurement—the cause is yet to seek. Any discussion of the values of the constants of the relation would be premature before further work has been done on the above features. It would help us in that work to receive samples of cotton from the field in an undesiccated state, or measurements thereon, comparable with samples sent for spinning test.

The graph is, however, sufficient to show that uncollapsed or swollen diameter is an excellent measure of intrinsic fineness and could be used to separate genetic and environmental variations. The special value of this quantity is that it can be measured on minute quantities of cotton, even on single hairs. The corresponding graph against collapsed diameter or ribbon-width shows the marked inferiority of the quantity as a criterion of fineness.

In measuring the swollen diameter, measurements have been confined to normal hairs, to minimise errors due to shape of cross-section. In a mixture containing immature hairs of fineness different from a more mature constituent, the value of swollen diameter will not be truly representative of the mixture. There is also an error in the calculation of Standard Hair-weight. Thus, as an instance, a mixture of equal numbers of hairs of Punjab American ( $H = 180$ ,  $M = 0.888$  from a count 40-20) and Desi ( $H = 240$ ,  $M = 1.043$  from an immaturity count 74-2) would yield values  $H = 210$ ,  $M = 0.966$  from a count 57-11. The standard hair-weights of the separate constituents are 203 and 230, mean 216; that of the mixture would be 209. The case is extreme, the error small and the interpretation of the values of a mixture uncertain. In a mixture of cottons differing in fineness, but not in immaturity, there is no such error.

*Separation of Variations of Strain and Culture.*—An illustration may be given of the way in which the standard hair-weight may be used to eliminate environmental variations in judging cottons for selection. A variety S.G. 29 was grown during four seasons at two

## STUDIES IN VARIABILITY OF COTTON.

places in Uganda. The following results were obtained in tests at the Institute :—

TABLE VI.

| Year.       | Grown at Serere. |             |     | Grown at Ngetta. |             |     |
|-------------|------------------|-------------|-----|------------------|-------------|-----|
|             | H.               | Immaturity. | Hs. | H.               | Immaturity. | Hs. |
| 1930 ... .. | 123              | 18—39       | 162 | 123              | 13—43       | 167 |
| 1931 ... .. | 170              | 54—11       | 177 | 164              | 50—15       | 176 |
| 1932 ... .. | 156              | 36—25       | 180 | 145              | 35—23       | 168 |
| 1933 ... .. | 150              | 33—24       | 175 | 150              | 29—24       | 178 |

Most of the apparent variation of fineness is eliminated when the hair-weight is corrected to the standard value. The remaining variation is probably due to extreme immaturity, so that the dead hairs are really less than  $0.45 \times$  the weight of "normal" hairs.

In another instance, Sakha 7, selected as a finer variant from Sakel, was tested.

TABLE VII.

| Year.       | H.  | Immaturity. | Hs. |
|-------------|-----|-------------|-----|
| 1931 ... .. | 120 | 59—5        | 121 |
| 1932 ... .. | 131 | 74—5        | 126 |
| 1933 ... .. | 137 | 66—6        | 136 |
| 1933 ... .. | 139 | 58—7        | 141 |

Despite the much smaller variation of hair-weight, the standard value indicates a probable coarsening.

Similar tests show that the Pump Scheme variety of the Sudan has been propagated for a considerable time with a constant fineness, despite some 20 per cent. variation of hair-weight.

TABLE VIII.

| Year.       | Grown at Talodi. |             |     | Grown at Kiriippi. |             |     |
|-------------|------------------|-------------|-----|--------------------|-------------|-----|
|             | H.               | Immaturity. | Hs. | H.                 | Immaturity. | Hs. |
| 1929 ... .. | 157              | 33—17       | 179 | —                  | —           | —   |
| 1930 ... .. | 162              | 46—9        | 172 | —                  | —           | —   |
| 1931 ... .. | —                | —           | —   | 164                | 50—14       | 175 |
| 1932 ... .. | 147              | 35—19       | 169 | 176                | 63—7        | 177 |
| 1932 ... .. | 163              | 52—13       | 172 | 162                | 52—13       | 172 |

Sections of the two 1932 samples shown in Fig. 2 illustrate the equality of perimeter or intrinsic fineness and the source of hair-weight variation in degree of wall thickening.



*Influence of Hair Fineness on Yarn Strength.*—One of the reasons for pursuing this method of separating the two factors in hair-weight variation was the impression that variations due to secondary thickening or immaturity did not have the same effect on yarn strength as variations of intrinsic varietal fineness. This was tested by calculating the correlation with highest standard warp counts of a set of 371 samples, the analysis of which is described more fully elsewhere. The coefficient of the correlation with the ordinary hair-weight is  $-0.68$ , with standard hair-weight  $-0.87$ . This is the highest value for any one character, that with effective length being  $+0.77$ . Other interesting results are that the correlation of hair-weight with length is improved from  $-0.47$  to  $-0.70$  and the low first order partial ( $+0.06$ ) on eliminating effective length from the correlation between  $H_s$  and  $M$ . It is the purpose of the method of expression to make these quantities independent of one another, as measures of the two independent factors, variety and environment. Little correlation is left between spinning value and maturity, when the effect of length, standard hair-weight, or both, are eliminated. This confirms a long-standing impression and means that spinning value may be judged from tests on cotton of varied maturity with more confidence. The more important effect of immaturity is on appearance rather than strength, a question which will be discussed elsewhere. The high second order partial coefficient between warp count and standard hair-weight when length and maturity are eliminated ( $-0.73$ ) and the lower partial with length ( $+0.37$ ) indicate that fineness of cell is the primary determinant of yarn strength, length the next, and secondary thickening a bad third. (This relative importance involves the actual range of variation of the quantities; for instance, the small effect of immaturity may be due to the fact that the samples vary about the condition for maximum yarn strength. Length may have more independent influence in a group of cottons in which the correlation  $EH_s$ .  $M$  is lower.)

*Interaction of Strain and Cultural Variations.*—In conclusion, another aspect of the separation of factors of strain and environment will be mentioned without specific data, as we have only incidental hints of its importance and desire field experiments to pursue its study. Nominally uniform strains may really be very impure genetically and the genetic constitution of the successful individual plants in a crop may vary with the conditions of culture. If the results of tests made on samples from a cultural experiment are interpreted as the effect of the conditions on one and the same type of plant, they may be very misleading. Cases have occurred where a more reasonable explanation

## STUDIES IN VARIABILITY OF COTTON.

would be that the conditions acted as a sieve to concentrate different variant types in the several samples.

## APPENDIX A.

*Treatment of Results for Bale Variations.*—The variations in length and hair-weight have been obtained from samples taken from 25 bales of Texas and 10 bales of Californian cotton. As stated in the text, two Shirley samples, each composed of about 70 tufts, were made from right and left hand sides of a layer near the top of a bale, and two more from a layer near the centre. The results were examined by analysing the variance, following the usual method developed by Fisher.

TABLE IX.  
ANALYSIS OF VARIANCE FOR HAIR-WEIGHT.

| Variance.                         | Degrees of Freedom. | Sums of Squares. | Mean Variance. | S.D.    | Log <sub>e</sub> S.D. |
|-----------------------------------|---------------------|------------------|----------------|---------|-----------------------|
| TEXAS.                            |                     |                  |                |         |                       |
| Between bales ... ..              | 24                  | 8499.54          | 354.143        | 18.8188 | 2.9348                |
| Between layers of same bale ...   | 25                  | 1099.50          | 43.980         | 6.6317  | 1.8918                |
| Within layers of same bale ...    | 50                  | 3339.00          | 66.78          | 8.1719  | 2.1006                |
| TOTAL ... ..                      | 99                  | 12938.04         |                |         |                       |
| CALIFORNIAN.                      |                     |                  |                |         |                       |
| Between bales ... ..              | 9                   | 9104.725         | 1011.636       | 31.8062 | 3.4597                |
| Between layers of same bale... .. | 10                  | 510.750          | 51.075         | 7.1467  | 1.9666                |
| Within layers of same bale ...    | 20                  | 1163.500         | 58.175         | 7.6273  | 2.0317                |
| TOTAL ... ..                      | 39                  | 10778.975        |                |         |                       |

TABLE X.  
ANALYSIS OF VARIANCE OF EFFECTIVE LENGTH.

| Variance.                         | Degrees of Freedom. | Sums of Squares. | Mean Variance. | S.D.   | Log <sub>e</sub> S.D. |
|-----------------------------------|---------------------|------------------|----------------|--------|-----------------------|
| TEXAS.                            |                     |                  |                |        |                       |
| Between bales ... ..              | 24                  | 142.66           | 5.9442         | 2.4381 | 0.8911                |
| Between layers of same bale ...   | 25                  | 29.50            | 1.1800         | 1.0863 | 0.0829                |
| Within layers of same bale ...    | 50                  | 72.00            | 1.4400         | 1.2000 | 0.1824                |
| TOTALS ... ..                     | 99                  | 244.16           |                |        |                       |
| CALIFORNIAN.                      |                     |                  |                |        |                       |
| Between bales ... ..              | 9                   | 110.4            | 12.2667        | 3.5024 | 1.2534                |
| Between layers of same bale... .. | 10                  | 9.5              | 0.9500         | 0.9747 | —0.256                |
| Within layers of same bale ...    | 20                  | 16.0             | 0.8000         | 0.8944 | —1.116                |
| TOTALS ... ..                     | 39                  | 135.9            |                |        |                       |

Using Fisher's Z test to compare the standard deviations, it is found that for both hair-weight and effective length the variation

between bales is much greater than either that between layers or that within the same layer. Also in all cases the inter-layer variation does not differ significantly from the intra-layer variation.

## APPENDIX B.

*Differences between Duplicate Tests.*—At the Shirley Institute duplicate tests are usually made on raw cotton. To obtain the magnitude of the errors and to detect any differences between staplers, the results of these duplicates were analysed. The cottons were divided into four groups according to their type. Each group was further subdivided into sets of cottons which had been tested by the same two assistants.

$$\text{The mean variance is } \frac{\sum (x_1 - x_2)^2}{2N} = v$$

where  $(x_1 - x_2)$  is expressed as the percentage difference between the results of duplicate tests and  $N$  is the number of pairs. The standard error of the mean is  $\sqrt{v}$ . The values of these standard errors for the various groups are given in the text.

The S.E.'s of hair-weight and effective length for 3 sets of exotic American cottons, each test having been made on a sample obtained from 32 tufts (instead of the usual 64), have also been calculated. There were 32 duplicate tests in all. Below are given the values of these S.E.'s together with the corresponding values for the 64-tuft sampling method.

|         |     |     |     | <i>Hair-weight.</i> | <i>Effective Length.</i> |
|---------|-----|-----|-----|---------------------|--------------------------|
| 32-tuft | ... | ... | ... | 6.27                | 7.37                     |
| 64-tuft | ... | ... | ... | 3.67                | 4.75                     |

These figures show that the adoption of the 64-tuft sampling method has greatly increased the accuracy of the results of the stapling test.

The grand means of each of the smaller sets of results were calculated for each character, all results within the set being made by the same two observers. The ratio of the percentage difference between these means and the corresponding S.E.'s was calculated in each case. There were 14 sets of cotton, and in each set the ratios were calculated for hair-weight, effective length, % number of normal hairs and % number of dead hairs, giving 56 ratios. If consistent differences occur between staplers a large proportion of these ratios should exceed 3.

## STUDIES IN VARIABILITY OF COTTON.

The ratios exceeding 3 were 3.35, 3.06, 4.21, 3.07 and 3.29. It consequently follows that there is no important difference in the technique of the various observers.

## APPENDIX C.

*Hand Stapling and Effective Length.*—For hand stapling tests on cottons a departure from the usual trade custom was made. Ten fringes of hairs were made for each cotton tested. These ten fringes were laid upon a velvet pad. Using a dissecting needle a line was drawn near each end of the fringe and the distance between these two lines was measured to the nearest  $\frac{1}{32}$  inch. The mean of the fringes was termed the staple length. The whole process took about ten minutes. The results from 36 cottons were examined, each cotton being tested for effective length by two staplers. An interval of a day was allowed to elapse before a repeat hand-stapling test was made.

TABLE XI.

|                                                      | <i>Sums of<br/>Squares.</i> | <i>Degrees of<br/>Freedom.</i> | <i>Mean<br/>Variance.</i> | <i>S.D.</i> | <i>Loge<br/>S.D.</i> |
|------------------------------------------------------|-----------------------------|--------------------------------|---------------------------|-------------|----------------------|
| Between cottons ... ..                               | 1126.61                     | 17                             | 66.27                     | 8.1406      | 2.0969               |
| Between hand stapling and effective<br>length ... .. | 44.50                       | 18                             | 2.47                      | 1.5716      | 0.4520               |
| Within effective length ... ..                       | 45.00                       | 18                             | 2.50                      | 1.5811      | 0.45813              |
| Within hand stapling ... ..                          | 9.00                        | 18                             | 0.50                      | 0.7071      | —0.3469              |
| TOTALS ... ..                                        | 1225.11                     | 71                             |                           |             |                      |

This shows that :—

(a) The difference between hand stapling and effective length results is negligible compared to the difference between the cottons.

(b) There is significantly less variation between replicate hand stapling results than between replicate effective length results.

It should be pointed out, however, that it remains to be shown whether hand stapling results show any trend over a long period of time. This can only be checked by making further tests on cottons which have been stapled by hand six or twelve months previously.

## APPENDIX D.

Correlation between Highest Standard Warp Count (X) and Hair Characters, Effective Length (E), Standard Hair-weight ( $H_s$ ),

Maturity Ratio (M) and (E), Hair-weight (H), Percentage of Normal Hairs (N). Analysis of 371 samples, including Sea Island, Sakel, Uppers, Exotic and Commercial Americans.

Total Coefficients.

|                 |          |    |          |
|-----------------|----------|----|----------|
| XE              | + 0.7712 | XE | + 0.7712 |
| XH <sub>s</sub> | - 0.8684 | XH | - 0.6820 |
| XM              | + 0.4135 | XN | + 0.4767 |
| EH <sub>s</sub> | - 0.7043 | EH | - 0.4724 |
| EM              | + 0.4878 | EN | + 0.5162 |
| MH <sub>s</sub> | - 0.3037 | NH | + 0.1303 |

1st Order Partial Coefficients.

|                    |            |      |            |
|--------------------|------------|------|------------|
| XE.H <sub>s</sub>  | + 0.4534   | XE.H | + 0.6966   |
| XH <sub>s</sub> .E | - 0.7198   | XH.E | - 0.5662   |
| XM.E               | + 0.0672   | XN.E | + 0.1441   |
| XM.H <sub>s</sub>  | + 0.3171   | XN.H | + 0.7800   |
| XE.M               | + 0.7165 + | XE.N | + 0.6975 - |
| XH <sub>s</sub> .M | - 0.8563   | XH.N | - 0.8538   |
| EH <sub>s</sub> .M | - 0.6686   | EH.N | - 0.6356   |
| EM.H <sub>s</sub>  | + 0.4049   | EN.H | + 0.6612   |
| H <sub>s</sub> M.E | + 0.0643   | HN.E | + 0.4958   |

2nd Order Partial Coefficients.

|                     |            |       |            |
|---------------------|------------|-------|------------|
| XE.H <sub>s</sub> M | + 0.3748   | XE.HN | + 0.3852   |
| XH <sub>s</sub> .EM | - 0.7275 + | XH.EN | - 0.7420   |
| XM.EH <sub>s</sub>  | + 0.1638   | XN.EH | + 0.5935 - |

Multiple Coefficient.

|                     |          |       |          |
|---------------------|----------|-------|----------|
| X.EH <sub>s</sub> M | + 0.9000 | X.EHN | + 0.9064 |
|---------------------|----------|-------|----------|

Errors of sampling and observation account for most of the residual variance after the multiple correlation, which has little real advantage over the total correlation on H<sub>s</sub> alone.



FIG. I.  
DIAMETERS AND STANDARD HAIR-WEIGHTS OF 33 COTTONS.

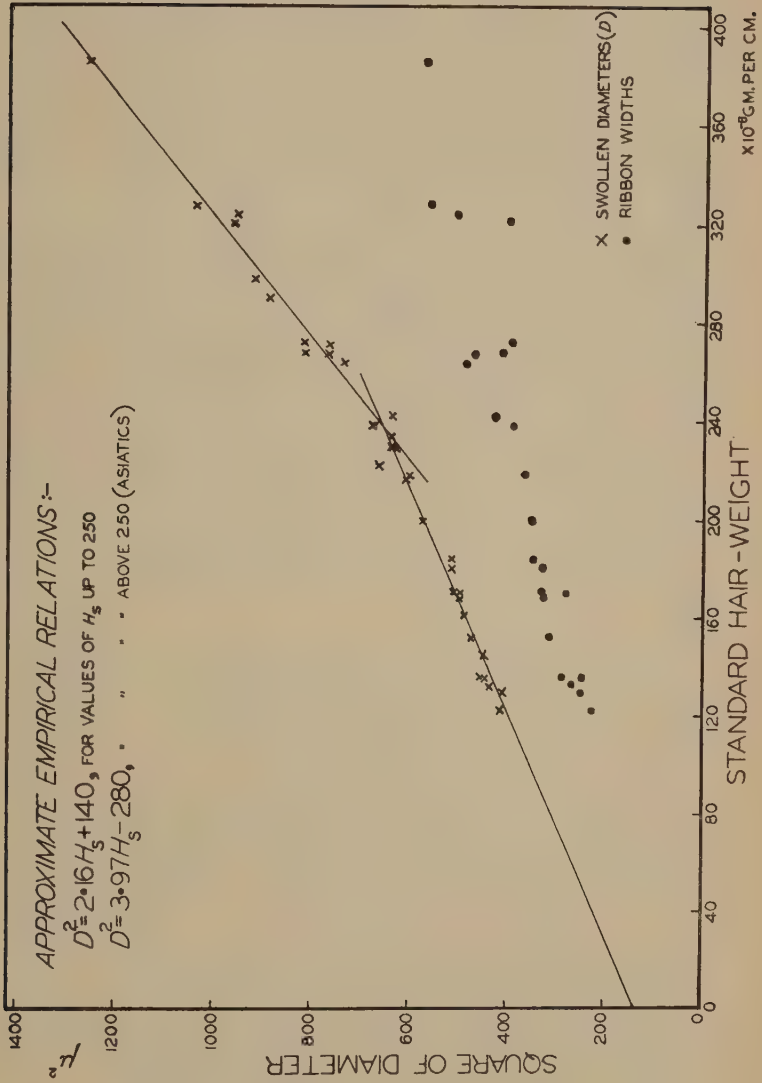
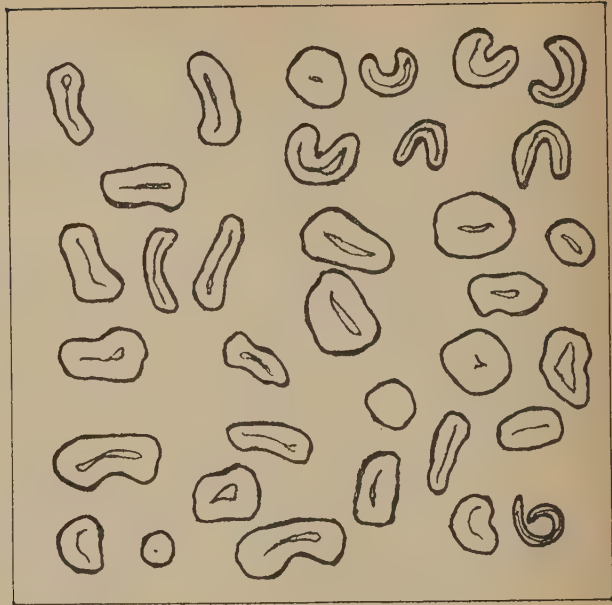
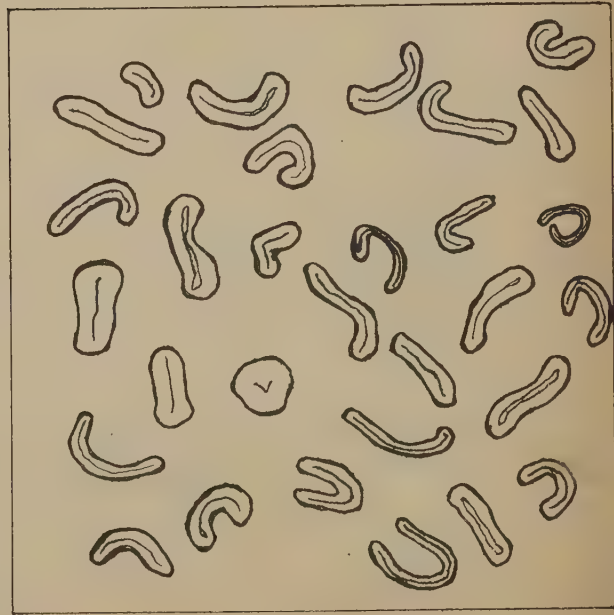


FIG. 2.

TRANSVERSE SECTIONS OF COLLAPSED HAIRS OF PUMP SCHEME STRAIN

TALODI  
1932

KIRRIPI  
1932



DR. F. T. PEIRCE, who introduced the paper, said that he and Mr. Lord had tried in the paper to bring together a number of very practical problems for consideration here and later. He wished to stress the word later, because they could be effectively pursued only by further consideration and action in the field. Many of the aspects of the subject concerned particularly the spinner, and, although such problems might not come to the notice of growers, their everyday work affected materially the demand, and therefore needed stressing in such a Conference where Lancashire and the Empire cotton growers met.

There were three practical problems in the question of variability : one was to reduce its amount, another was to determine the correlations between the variants, and the third, the scientific aspect, was that of finding invariants, or, in other words, of expressing observations in such a way that they were not subject to unessential variations, so that variations in the figures were of significance.

He dealt firstly with the question of reducing variability. The spinners demanded uniformity of material for their technical purposes : that was rather Dr. Turner's province. Then there was the question, more technical to themselves at the Shirley Institute, of getting results with a minimum amount of labour. That was a growers' problem also, and it was one in which the Shirley Institute could co-operate.

He wished to bring up again a point made in an earlier discussion that as far as possible the two sets into which all the variations may be divided should be separated ; these were strain and culture. The Institute often received material which was intended to give information on the characteristics of, and the differences between, strains, but which had very great differences which were only cultural, which varied from season to season, and were accidental to the particular sample of the strain. If the maturity in the strains varied from year to year in different combinations, it became almost impossible to get satisfactory results.

One way in which this could be overcome was by studying the variability of the product in the field. He did not know how the characteristics of the cotton hair varied in the crop, and whether it was practically possible to pick a crop which was very uniform from the point of view of culture in order to provide material by which the characteristics of the strains could be determined more accurately and with less uncertainty and labour.

Dr. Balls had described his method of reducing this variability of the developmental stage of the cotton hairs, and it seemed to be a very useful one. Perhaps it might be possible to discover whether one could pick from a crop, by signs and criteria which could be seen in the field, a sample of cotton which was properly matured, and thus secure that there was very little difference in the degree of maturity of the different strains which came up for test.

Proceeding further, using the uniform strain, the effect of degree of maturity and of cultural variations on the characteristics could be studied, keeping the strain constant. This was commonly done, although the material submitted for examination had not as definite a history as was desirable; that would require a systematic series of samples of one definite strain in which the cultural conditions had been altered so as to effect in a gradual way the maturity of the strain.

Dr. Peirce said that he recognised the practical complexities and difficulties of his demands. There were, of course, administrative difficulties in getting pure and uniform cotton, from the point of view both of purity and maturity of strain. It had been said in a previous discussion that a pure strain was a very difficult entity to produce.

At the end of his paper he had raised another point, namely that in cultural experiments one could not be certain that the strain was uniform at the end of the experiment, even though it had been at the beginning. They would be glad to get material to enable that point to be studied, namely the sieving action of culture on a strain which was really not genetically pure, so that the same mixture of seed sown in different conditions would produce crops which were really from seeds of different genetic constitution.

The main subject dealt with in the paper was another method of minimising variability due to the complex of strain and cultural variations. They had to handle material in all states of maturity, and so they tried to work out, from the point of view of characterising a strain, the invariants of the strain, the characteristics which varied as little as possible with the culture—and to separate them from variables which could be regarded as indices of the cultural conditions and the degree of development.

At the Shirley Institute they worked ordinarily on three characteristics obtained from the combined stapling test: the length, the hair weight and the immaturity count. Broadly speaking, the cotton hair could be characterised by three geometrical quantities: the original cell diameter or cell perimeter, the length, and the degree

of thickening. There might be other variable characteristics apart from those geometrical ones, but he was disinclined to pursue those to any great extent until the three fundamental geometrical features had been thoroughly correlated and their effect on spinning quality and on the value of the cotton determined.

The three quantities measured by the stapling test, however, were not *directly* the three geometrical fundamental quantities, and a method had been worked out by which a closer relation between the two sets of variable features could be determined. The length was straightforward; that was measured directly; it was of very great importance and was a fundamental feature of the cotton. The immaturity count was directly related to the degree of thickening as a geometrical feature, but on an arbitrary basis. One feature of the method put forward in the paper was that it made that arbitrary basis absolute, so that by any method which might be used to determine the degree of thickening the resultant figures could be compared.

That fundamental geometrical feature could be expressed most simply as the ratio of the actual area of the section of the hair to the area of the circle with the same perimeter. It was not used in precisely that form in the paper, because one did not get hairs which were completely filled in that way, and the figures should as far as possible be directly comparable with the familiar quantities which were obtained from tests. The basis of the degree of thickening measurement, therefore, had been made that degree of thickening which was regarded as representing a normal crop. There was nothing absolute about it, but it could not be far wrong, and that degree of thickening actually did represent a normal, well-developed crop fairly precisely.

Then it was necessary to separate out the two factors which came into the all-important measure of hair weight. This measure was used more than any other quantity; it had proved itself of the highest significance, and yet it was definitely a complex of two totally unrelated features as far as both the biological origin and the geometrical form were concerned, and, in his view, as far as the spinning quality and behaviour of the cotton were concerned; those two features were the original cell diameter or perimeter, and the degree of thickening. The degree of thickening was found from the immaturity test, and it was simple, if one had the material on which to get the data, to correct the actual observed hair weight to a hair weight corresponding to normal development, and then separate the question of the characteristic hair weight of the strain from the accidental factor imposed on that by the varying degree of maturity of the crop.



Anticipating a possible criticism of the method, Dr. Peirce said that the sound statistical way would be to get from a number of strains sets of samples of as pure strain as possible grown under different conditions, so that the degree of maturity varied, and then correlate or get the regression line of hair weight on the degree of maturity. That was attempted, but the material used was not very satisfactory, and so recourse was had to another method.

In that second method which was put forward in the paper, tentatively and for discussion, that of the standard hair weight and the maturity ratio, the relative hair weights of the different types had been measured, and so, from the actual hair weight, they had calculated what the hair weight would be for any maturity count. There was implied in that calculation an assumption that was not true, but it was satisfactory in practice, though it could be improved, even from the practical point of view.

The immaturity count gave a frequency diagram with three very broad intervals, and evidently, if the frequency distribution varied, the distribution of maturity within those three groups also varied; that is, if the cotton were very highly mature, with a great number of normal hairs, one would expect that there would be a bigger proportion among the normal group of very mature hairs, and that the average of that normal group would be heavier in that very highly mature cotton. He and his colleagues needed the material to make that further refinement of the method—to combine the two methods—the regression and the calculation, by calculating the maturity coefficient from the immaturity count and correlating the hair weight with that in a given strain. It would both test the method and also give a further refinement to it. That was a direction, therefore, in which the cooperation of the growers was needed in order to get samples of the same strain of different degrees of maturity.

#### DISCUSSION.

DR. PICKARD *explained that at the Shirley Institute Dr. Peirce was in charge of the Department that dealt with the laboratory measurements carried out on cotton hairs, while Dr. Turner supervised the Spinning Tests Department, and that the kind of samples for which Dr. Peirce had asked were small samples for laboratory examination, and not the larger samples required for spinning tests.*

MR. HOSKING *asked if there were any advance in the knowledge of the part played by the coefficient of friction between the hairs since the publication of Dr. Balls' book on "Quality in Cotton."*

*As regards Dr. Peirce's request for samples, he referred to the limitation which had been placed on the number of samples to*

be sent, and mentioned the lack of any relation between the number of samples tested, as set out in the paper by Mr. Underwood, from the different cotton-growing countries, and the production of those countries as shown in the table at the end of the Empire Cotton Growing Corporation's Annual Report.

At Serere they had numerous trials which were all picked at weekly intervals: if those weekly interval picks would be of any value to Dr. Peirce, he would be very glad to send him samples. They also had replicated variety trials at ten centres in the Eastern Province, and he asked if the cotton from those trials would be of any use to Dr. Peirce as possibly showing the effect of place and different climate on the same variety.

Mr. Hosking said, in reference to Dr. Peirce's request for "normally matured" cotton, that it was impossible to say what was meant. As an instance of the difficulty of defining the term, he said that weekly picks were made on their pedigree lines, and individual lint measurements were made every week on every single plant. During last season, variations from 22 to 36 mm. on the same plant were common from week to week. The actual variation from pick to pick was often immensely greater than the difference between individual strains. Dr. Peirce might also have some interest in the cotton picked at weekly intervals from those pedigree lines.

DR. PICKARD explained to Mr. Hosking that the limitation which the Corporation, at the request of the Shirley Institute, had placed on the number of samples sent for spinning tests was due simply to the fact that they could not deal with more than their resources permitted. That was quite a different matter from the small samples for laboratory tests for which Dr. Peirce had asked. He did not think that it would probably be necessary, at all events at present, to make any limitation in the number that could be sent for that purpose.

Continuing, DR. PEIRCE said that the material they received was such a complex of differences of strain and differences of culture that it was very difficult to get correlations of either. They would therefore welcome any samples which would give data on those two separate correlations. In such analytical and developmental work it would be well to keep to well-known and important strains in order to minimise the work.

Dr. Peirce added that he would also be glad to discuss the possibilities of field criteria for determining the state of the cotton lint itself, and it was important to work those out from the practical point of view. If there were such criteria, they at the Shirley Institute would be able to obtain material in different states of maturity, while on the other hand, when they were worked out and those correlations determined, growers would be able to make discriminating pickings. Whether discriminating picking in the field was impracticable he

did not know, but it was worth studying to see whether by observations in the field one could pick cotton of full maturity ; and to see the way in which length and the degree of thickening separately, and possibly the cell diameter, varied with culture, and with the boll position ; whether, for example, the first boll of a monopodium were more fully matured than the top boll of the main stem.

In making this request for samples, Dr. Peirce remarked that it might result in hundreds of samples, with which they could not possibly deal ; and in that event they would be obliged to discriminate and see which samples had the data and the origin which would give the information that they were seeking.

On the importance of the coefficient of friction, in determining quality, Dr. Peirce attached importance to three characters only ; the first was the intrinsic fineness, the second the length, and the third the degree of thickening. It was unquestionably possible to maintain that other features had a very big influence independently of those. He laid emphasis on the word "independently" ; undoubtedly they had a considerable effect directly, but the question was to what extent it was independent. For instance, the coefficient of friction, or the clinging power of the cotton hair, certainly depended very largely on the convolutions and on the surface wax and on the cell diameter. To what extent were they independent of the three geometrical factors ? How far could they vary if those three geometrical factors were identical ? That was a question to which no answer could be given until more work had been done on the correlations with the three geometrical factors themselves. Another question was to what extent was a high wax content due to a heavier coating of wax per unit surface of cotton, and to what extent was it due to the geometrical features of the cotton itself, the way in which the wax was distributed on the surface, and the ratio of surface to weight ?

Replying to a question by Mr. Hosking, Dr. Peirce said that the samples required were seed cotton, in preference to lint.

DR. BALLS said that one point which seemed to him to emerge clearly from the discussion was that Mr. McConnel's original scheme of a free interchange and seconding of staff between the Empire Cotton Growing Corporation and the Shirley Institute for study periods was becoming more desirable than ever.

He hoped growers would bear in mind the fact that hair investigation and spinning tests were tedious and costly, and that they should therefore endeavour to send for test only wholly unexceptionable material. In Egypt they sent nothing for spinning test that was not derived from chequer-board plots, the repeat plots having all been thoroughly mixed so as to provide a representative sample of the whole area. The picking of that area, moreover, was done at

as short an interval as was practicable, though unfortunately, even under Egyptian conditions it was not possible to cut down the picking period on the yield and spinning test chequers to the same degree of fineness that was used for the purity chequers of which he had spoken on the first day of the Conference.

There was one suggestion that he would like to make to Dr. Peirce, and that was that he should consider the practicability of using the term "hair maturity" and not simply "maturity." "Maturity" to the cotton grower covered such a multitude of sins; even "hair maturity" covered many. If it were possible to speak of "hair maturity," he thought it would be less confusing.

As to what that maturity consisted in, there were two separate and distinct things: one was the number of growth rings, and the other the thickness of the individual growth rings. In practice undoubtedly it was both. Moreover, really adequate hair maturity, as Dr. Peirce used the term, might represent a very undesirable condition, such as that from which the Egyptian crop suffered in the previous season on account of the abnormally cool summer; in other words, excessive thickening of the hairs. The mean hair thickness was then up by between 10 and 15 per cent.—that is to say the cotton was too coarse. A thoroughly matured crop might therefore be unspinnable. The crop might be said to be balanced on the peak of a steep-sided skew curve; if it went the least bit too far in the way of fineness, large numbers of immature hairs and nep were produced; if it went too far down the other side in the direction of "well-matured" cotton, it became coarse and was not as good as it should be.

For the purpose of testing these difficulties, of discriminating between environmental and constitutional effects, he thought there was nothing to beat the method of weekly pickings, as Mr. Hosking had suggested, or even picking at shorter intervals. He had worked with daily pickings; the results were contained in No. 107 of the Giza Technical Bulletins, and the graphs, which were typical of cotton grown under quite uniform irrigated conditions—almost ideal conditions—showed that there was an enormous and systematic swing in all, or nearly all, those hair properties. He said nearly all those properties, because later work which Mr. Brown and Selim Effendi were doing, and which it was proposed to extend very largely with their new spinning test equipment, had shown that two factors in the hair properties were comparatively invariable. The first of those was that of which Dr. Peirce had stated that he had found the same thing—the perimeter of the circle, or what was called for convenience the original cell diameter. The diameter of the hair thus appeared to be a specific character which varied very slightly, whereas the amount of thickening laid down on the inside of that hair, thereby affecting the hair weight, varied enormously. The



other character to which he referred was the intrinsic strength; although the work which they were doing in Egypt had not gone far enough to justify dogmatic statements, he thought it would be found that the intrinsic strength of the hair—i.e. the breaking load of the hair for an equal weight of cellulose—was a specific character, and one which varied tremendously from strain to strain.

He could not agree with Dr. Peirce that hair properties, other than the three he had specified, were relatively quite unimportant. That was not true of what he had called the intrinsic strength; in many respects that was the most important character of Egyptian cotton, and when it came to tyre-making, the dominant character of the cotton was the intrinsic strength.

DR. PICKARD asked Dr. Balls whether by "cotton" he meant "yarn," because it was the strength of the yarn which counted in the tyre.

DR. BALLS replied that the intrinsic strength of the hair determined whether the cord would stand up to such a job as that which it had to do in a tyre: a cord was synonymous with a cotton hair.

MR. PARNELL said that he was rather disappointed, when he visited the Shirley Institute on his last leave, to learn that the samples that he was sending were not what they really required. He understood that commercial pickings were more favoured than the samples that he was sending, which consisted of cotton picked over as short a time as possible, and taken from only undamaged bolls. He gathered from what Dr. Peirce had said, however, that he must have been under a misapprehension, and he was very glad, because he would like to send special samples that were as uniform as possible. He particularly wanted to ascertain what was the effect on the hair characters of the great variations that took place in the climate at Barberton during the course of a season. He believed that the effect of these climatic differences was hardly sufficiently appreciated. He asked Dr. Peirce whether samples sent for examination with this object should preferably be from single plants, or from several plants of the purest strain possible. Samples from single plants would make it quite certain that there were no variations due to strain differences. He mentioned further that he would also like those samples to be tested for the degree of thickening, as distinct from the proportion of thickened hairs. At present little information was available on this point, and he thought that data regarding the actual degree of thickening of the lint hairs would be very valuable when considered in connection with the varying climatic conditions during the period of growth.

MR. SAMPSON suggested that when small samples were sent home for the examination that Dr. Peirce proposed to carry out, they should be accompanied by full notes as to climatic conditions, degree of insect attack, and any other relevant facts that might usefully be considered in connection with the results obtained.



## STUDIES IN VARIABILITY OF COTTON.

PROFESSOR ROBERTS asked whether Dr. Peirce had carried out any tests of this kind on Punjab-American cotton. He would like to know, for example, whether there was evidence of any difference in hair weight per centimetre of American cotton grown in the Punjab as compared with similar cotton grown in the U.S.A.

DR. PEIRCE said that he agreed with Dr. Balls that the term "maturity" was a bad one, but he was not responsible for its introduction. He preferred "degree of thickening," which was a term he thought would be generally understood, but he used that term himself rather more specifically for the geometrical definition. However, the term "maturity" seemed to be established.

Then there was the question of the factors in hair maturity or degree of thickening, the number of the rings or the thickness of the individual rings. Differences in the degree of thickening could occur in both ways; one could have a crop growing for a full period, but at a rather subdued rate of deposition, or on the other hand, one could have good deposition cut short. He imagined that agriculturally that was quite possible, and might lead to distinct differences. Crop data with all samples would be extremely useful.

Speaking further in reference to the samples for which he had asked, Dr. Peirce said that samples for his work were necessarily different from those required by the Spinning Department, to which the immediate commercial value of the crop was undoubtedly of great importance. Their results represented the crop which might be got from that growing area, and for that purpose the representative crop bulk sample was essential. To separate out, from the point of view of understanding their effects separately, the strain characteristics and the effects superimposed on the strain of degrees of maturity or thickening was quite a different problem. It was true that a cotton could be over-thickened, but that was not likely to occur in Uganda, South Africa or the Punjab. The effect was quite definite, and that was one of the reasons why he did not use the full circle as the basis of expressing the maturity ratio, but rather what might be regarded as a good commercial degree of thickening.

Mr. Parnell had raised the question of the degree of thickening as distinct from this immaturity count. The immaturity count was a frequency distribution; it was a broad one, but good enough for their purposes. By cutting sections, the relationship of the three types—normal, thin-walled and dead hairs—to the continuous distribution of the degree of thickening as expressed geometrically had been determined; otherwise he did not think there was any distinction between the degree of thickening and the immaturity count.

Replying to Dr. Balls, Dr. Peirce said he had not meant to imply that specific strength did not vary; the finer cottons were intrinsically stronger. The question remained, however, whether

such variations as occurred were really independent of the geometrical variations. One could get two cottons with the same hair weight, one of which had a bigger diameter and a lower degree of thickening ; that cotton would have a different specific strength from that of a finer cotton better matured, but that difference in specific strength would not mean the introduction of an independent factor. There might be independent factors, but it was so difficult to separate them that he did not see any positive evidence at present that they were important.

In answer to Professor Roberts, Dr. Peirce added that they did not like to do any theoretical work on Punjab American cotton, owing to the prevalence in it of *Desi*.

PROFESSOR ROBERTS said that samples of unmixed Punjab-American cotton could readily be obtained from the Agricultural Department.

DR. PICKARD, replying to Professor Roberts, said that comparatively few tests had so far been carried out at the Shirley Institute on Indian cottons, because India had its own Technological Laboratory where such work was being done. During the last year or so, however, increased interest was being taken in Lancashire in cottons from India, and samples were now being received at the Shirley Institute direct from that country. For the purpose of the fundamental studies of the cotton hairs, the Shirley Institute would be glad to receive samples from India for comparison with the samples obtained from other parts of the Empire.

DR. TURNER mentioned that some years ago he had tested samples of Punjab-American 4F, grown at Lyallpur under various conditions, and the difference in hair weight between those samples ranged from approximately 97 units to over 200 units. They had recently tested at the Shirley Institute samples of Punjab-American cotton in which there was a difference in hair weight of about 25 per cent., and a corresponding difference in maturity figures. He quoted these cases in order to give some idea of the range of variation that occurred in the Punjab itself.

DR. AHMAD said that they had for the past two years been carrying out at the Technological Laboratory, Bombay, direct maturity counts on samples of cotton, and had found them of increasing importance. He considered that the method now suggested by Dr. Peirce was a valuable step forward. He had some fear lest, in attempting to obtain the maturity ratio for what Dr. Peirce called the standard hair weight, such differences as occurred between one variety and another might be eliminated altogether, or at all events greatly diminished. He noted in Table V of Dr. Peirce's paper that, whereas the differences in the maturity count were fairly large, differences in the maturity ratio were small, and he would therefore like to know whether Dr. Peirce definitely recommended that the maturity ratio should be calculated in the case of cottons the complete previous history of which was not definitely known.

*Referring to remarks made by Dr. Balls earlier in the discussion, Dr. Ahmad mentioned that recently two samples from a pure line had been examined, and whereas one had given a hair weight per centimetre of over 200 units, the other had given a corresponding figure of only 140 units. The one sample spun 30's, and the other only 13's. He mentioned this as an instance of the great difference that was possible in a pure line, both in hair weight and in spinning performance.*

DR. PEIRCE replied to Dr. Ahmad's question in the affirmative. The calculation was intended to turn an arbitrary figure determined from a microscopic test involving personal judgment into a geometric measurement; it gave a close enough approximation in practice. That approximation depended on the uniformity of those ratios under Normal, Thin-walled and Dead on page 7 of the paper. Strictly speaking one should regress them, but it was near enough to take them as uniform. He would prefer to relegate any regression method to the next step. The expression in the paper could be regarded merely as a method of weighting the three quantities in order to get one mean figure for the maturity of the sample. Then one could go back to the first method discussed, which was discarded—that of regressing the hair weight and the measure of immaturity—and put the maturity ratio in place of the immaturity, and then regress the hair weight on the ratio of maturity. That would be the logical and rigorous way of doing it in order that it could reasonably be said to be general for all strains and all degrees of maturity. He saw no necessity for any specific difference of treatment between one strain and another in reducing the immaturity figure to what was effectively a geometric measure.

MR. TROUGHT said he would be quite willing to cooperate with Dr. Peirce, and try to work out criteria by which uniform fully thickened samples could be obtained such as Dr. Peirce wanted. Some of those criteria were obvious; but only Dr. Peirce's tests could show whether those criteria were correct and would give the type of sample he required.

DR. BALLS referred to the point raised by Dr. Ahmad as to the variation of samples from one lot to another. A field-to-field difference in hair weight was an everyday occurrence, and variations of the order of 50 per cent. inside the same pure line or commercial crop were of everyday occurrence, even in Egypt under irrigation and on the same estate. One help in correcting that variation was the determination of seed weight; seed weight and hair weight were closely correlated, and in the Bulletin to which he had already referred he had called attention to the dimensional relation between the two. Since that Bulletin was published, Mr. Brown and he had worked out a method which was in regular use at Giza, by which the target diagrams and purity chequers were plotted for the square root of the hair weight and the cube root of the seed weight, thus bringing both quantities down to the same linear dimensions. The correction thus applied had proved most useful.

DR. PEIRCE thanked Mr. Trought for his definite offer of cooperation ; it needed careful discussion for there were obvious dangers : one might easily get criteria which would select long or short hairs rather than mature hairs.

*He thought there was a good deal that could be done in the field in the way of exploratory work on the relation between hair characters and field data. It was possible to develop quite an effective judgment of length. The diameter of the uncollapsed or swollen hair was not very difficult to measure, and it might be used considerably in the field in exploring the variations of length and original cell diameter.*

### THE MOST TROUBLESOME IMPURITIES IN COTTON.—BEARDED MOTES AND NEPS.

BY

GLADYS G. CLEGG (*Shirley Institute*).

In 1930 a paper read to this conference on the subject of "Immaturity of Cotton" gave some experiences of the staff of the Shirley Institute with regard to immaturity and neppiness in raw cottons, processing, and finished goods. That paper was an introduction to the subject, given with the object of emphasizing the responsibility of the grower for this particular trouble of the spinner. The subject is to be continued to-day under the title "Bearded Motes and Neps." It is proposed to discuss the matter in much greater detail, and to describe various types of neps, the relation of the immaturity of the raw cotton to the neppiness of yarns, and dyed and finished cloths. The possible formation and removal of nep will also be discussed and a brief survey given of the immaturity and incidence of nep in the Empire Cottons since 1930.

With regard to the other impurity which has been introduced—"the bearded mote"—some account will be given of its incidence in the finished cloth together with a description and classification of the types of mote, its relation to the trash content of the raw cotton, and difficulties of its removal in processing.

Before giving a detailed classification of neps and motes it will be necessary first to define the terms as they are employed in the trade. The term "nep" is used both in the cotton and the wool trade to mean a small knot or cluster of fibres, and a "mote" to mean "A black spot in the yarn or woven cloth due to a small piece of seed coat." Since in most cases the motes are quite obvious both in the raw cotton and the grey cloth, and can in many cloths be removed in



the finishing processes, they do not present such an acute problem as that of nep which often shows itself for the first time in the finished cloths. It is not always possible, however, to remove motes in the finishing processes; for example, in cloths which are partly rayon, only a light finishing treatment, not sufficiently severe to remove motes present in the cotton yarns, can be given. Because of the development of such mixed cloths, yarns free from motes are in greater demand than they have been in the past.

### NEPS.

*Types of Nep.* It is quite obvious that all tangles and clusters of fibres in the yarn and cloth cannot be attributed to faulty raw cotton: some of them are formed during ginning or in the preparation for spinning. It has been found possible from a microscopic examination of the neps to say to some extent which of them were present as actual nep in the raw cotton and which as potential nep, and which have been formed during the processing of the raw cotton. In order to make this classification, the technique of the immaturity test is employed. The nep is placed on a slide, teased out and treated with 18 per cent. caustic soda and the type of hairs constituting the nep observed.

A large number of neps from raw cotton, yarns and finished cloths have been examined in this way, and the following classification has been arrived at:—

1. *All dead or homogeneous neps.* These are small tangles of dead hairs all at the same stage of development and must have been formed from a small patch of dead hairs growing together on a seed.

2. *Immature and mixed neps.* Immature neps are caused by the tangling of dead, thin-walled and normal hairs. If there is a nucleus of fuzz or foreign fibre they are usually described as mixed.

3. *Process neps.* These are neps composed of tangled normal hairs, and are formed in processing.

4. *All fuzz neps.* A nep of this type consists of a tuft of fuzz hair.

Of these four types the homogeneous dead nep is the only one which can be said to be pre-existent in the raw cotton. It is there either as a nep or in such a form that any processing at all will cause it to nep up. How and why these patches occur we do not know—



that is a problem for the grower. It is improbable that they have arisen from the immaturity of a whole seed, as they are so often observed as tiny specks in the raw cotton, whereas, when all the lint on a seed is immature, it usually forms what are commonly called "rats tails" — stringy looking tufts of immature or dead cotton.

The immature or mixed neps vary in number with the ginning and processing, although the fundamental cause of their formation is the immaturity of the raw cotton.

The process nep is the result of faulty preparation for spinning and the all fuzz nep probably originated at a fuzz fragment of seed coat from which the latter has been removed in finishing.

*Incidence of neps.* The incidence of the various types of neps in the finished cloths is interesting. Cloths of various types are sent to the Shirley Institute regularly for examination of "white specks after dyeing." Taking 30 cloths at random the various types of nep were found to occur in the following percentages:—

|                           |        |     |
|---------------------------|--------|-----|
| Homogeneous dead neps ... | ...    | 60% |
| Immature and mixed neps   | ...    | 20% |
| Process neps              | ... .. | 10% |
| All fuzz neps             | ... .. | 10% |

A classification was made of the neps in the yarns spun from Empire cottons, with the following results:—

|                           |        |     |
|---------------------------|--------|-----|
| Homogeneous dead neps ... | ...    | 54% |
| Immature and mixed neps   | ...    | 42% |
| Process neps              | ... .. | 4%  |

In both the Empire yarns and the finished cloths the homogeneous dead neps are obviously the most tenacious, and particularly in the finished cloths. Immature and mixed neps are present in both, but in a much smaller proportion. The explanation of this larger proportion of immature neps in the Empire yarns (which include many experimental growths) may be due to their general immaturity being on the whole greater than that of ordinary commercial cottons. On the other hand, many of the immature and mixed neps are formed late in the spinning processes, and are probably not so firmly entangled as the homogeneous type; it is possible that they become detached and are removed in weaving or its preparatory processes.

*Relation of Neppiness to the Immaturity Test.* The next question to be considered is the relation of the immaturity of the raw cotton to the neppiness of the yarn and the appearance of the finished cloth. The factors influencing nep production are:—(a) Quality of raw cotton; (b) Conditions of processing. The latter scarcely affects the grower. Blowroom treatment (the number and speed of beatings), carding (rate of production, type of clothing, settings, etc.), and percentage removed in combing are all factors in processing which affect the amount of nep.

The effect of the immaturity of the raw cotton will be dealt with in detail. In order to examine its effect on incidence of nep in yarns, 8 groups of cottons and their corresponding yarns were chosen from spinning test samples spun in the ordinary routine.

|      |                |      |
|------|----------------|------|
| 1931 | Uganda         | 50's |
| 1932 | „              | 50's |
| 1933 | „              | 50's |
| 1931 | Sudan American | 60's |
| 1932 | „ „            | 50's |
| 1933 | „ „            | 50's |
| 1933 | Tanganyika     | 50's |
| 1933 | Nigerian       | 50's |

The yarns were wound on cards and placed in order of neppiness by at least three independent observers. The maturity ratio\* was plotted against the order of yarn appearance, No. 1 being the least neppy in each set. The results are plotted in Fig. 1. Seven of the eight sets indicate a definite tendency for the yarns to become more neppy as the maturity ratios decrease.

In the Sudan American group, two of the most mature cottons produced the neppiest yarns. On examining the neps they were found to be chiefly seed coat and when these were discounted and the number of dead and immature neps plotted against the corresponding maturity ratios this tendency of neppiness to increase with immaturity was again evident (Fig. 2).

So far the results show a relation between cotton immaturity and yarn neppiness within the same type of cotton. In order to see whether the type of cotton has any effect on the production of nep, two yarns from each of the sets were taken and windings made, making 16 in all, and these were again arranged in order of neppiness. The results are plotted in Fig. 3., and the same trend is again apparent. On correlating the maturity ratio and neppiness (by finding the

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\* A suggested new method of expressing the maturity of a cotton. For explanation see paper for this Conference by F. T. Peirce and E. Lord.

correlation coefficient between rank and variate and transforming this coefficient into the correlation between actual variates) a coefficient of  $-0.714$  was obtained. (For 16 pairs a correlation of  $-0.497$  is on the 5 per cent. level of significance.)

An experiment is at present being carried out at the Institute on the relation of immaturity of the cotton to the appearance of the dyed and finished cloth. The experiment is not yet complete, but, so far, one definite result has been noted—the two cottons with the highest maturity ratios have given both the neppiest grey yarns and the neppiest dyed yarns—a result contrary to our experience for the other sets of grey yarns. This raises an important point with regard to incidence of nep in raw cotton.

On various occasions, dyed cloths have been sent to the Institute which are very much neppier in appearance than any of the cloths woven and dyed from Empire cottons, although one of the latter used in the test gave an immaturity count of 28-35, as low as any we have had. Stripping and re-dyeing of these cloths, with the dye used in the test, did not affect the number of visible neps.

What then is the significance of the immaturity count in relation to neppiness? The immaturity count gives a figure for the general degree of thickening of the cotton. It is conceivable however that a cotton may have the majority of its hairs normally thickened, giving a high maturity ratio, but its 10-20 per cent. of dead hairs may be distributed in small patches through the cotton in such a manner that they nep up and are not removed in processing and thus survive through to the finished cloth. Another cotton which is generally immature may have the same percentage of dead hairs disseminated through the sample with the result that they get through the processing without nepping up.

It would appear therefore that there are conditions of neppiness unaccounted for by the immaturity figure, and since in the cases mentioned, and in many of the cloths submitted to us, the majority of the neps are homogeneous dead cotton neps, it would appear in many cases to be the sporadic presence of these small patches of dead hairs, scattered throughout the sample, which is the real cause of the trouble to the dyer and finisher.

The cause of this type of nep is another problem for the grower. Where, and how, are these small tufts of dead hairs produced? Is the cause mycological, physiological, or genetic? Are some strains more likely than others to produce these small tufts of dead cotton?

## IMPURITIES IN COTTON—MOTES AND NEPS.

A brief summary of the immaturity and neppiness of the Empire cottons during the years 1930-1933 is given as follows :

| <i>Cotton.</i>                                                                                                     | <i>Maturity Ratio<br/>of raw cotton.</i> | <i>Report on neppiness of yarns.</i>                                                                                                                                                                    |
|--------------------------------------------------------------------------------------------------------------------|------------------------------------------|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Nigerian, 1931-32   ...   ...<br>1932-33   ...   ...                                                               | ·874<br>·930                             | There is a general improvement apparent. Strain C which shows a general tendency to nep was not sent in 1933.                                                                                           |
| Tanganyika, 1931   ...   ...<br>1932   ...   ...<br>1933   ...   ...<br>409-415   ...   ...<br>416-421   ...   ... | ·842<br>·902<br>·902<br>·901             | No apparent improvement in the amount of nep. The verdict has always been "fair" for these cottons.                                                                                                     |
| Sudan American, 1931-32   ...<br>1932-33   ...                                                                     | ·889<br>·923                             | The strains showing an obvious tendency to neppiness have been eliminated from tests and there is a definite improvement since 1930 largely due to this.                                                |
| South African, 1932   ...   ...<br>1933   ...   ...                                                                | ·953<br>·939                             | These yarns are always free from nep.                                                                                                                                                                   |
| Uganda, 1930   ...   ...   ...<br>1931   ...   ...   ...<br>1932   ...   ...   ...<br>1933   ...   ...   ...       | ·751<br>·967<br>·889<br>·906             | 1930 was described as very neppy. 1933 an improvement on 1932 but the latter was much worse than 1931. The variation is apparently due to the climatic conditions prevailing during the growing period. |

It should be noted that the general verdict on yarn neppiness bears some relation to the maturity ratio of the raw cottons.

## MOTES.

The next impurity to be considered is the mote, defined as a small black speck due to a piece of seed coat surviving through to the finished cloth. Again the two factors involved in its survival are the raw cotton and processing. A large amount of work, some of which has been published, has been done by the Shirley Institute in the last few years on the nature and removal of impurities and waste. Waste tests have been carried out in the mills on many types of cotton. A known weight, some 100 lbs. of the mixing usually employed in the particular mill, was passed through the usual combinations including the card. The droppings at every machine were separated, collected, weighed, and fractionated into lint, seeds, chaff, dirt, and dust.\*

\* The constitution of the trash is as follows :—

*Seeds.* There are seldom any normally developed seeds in this fraction. There are large pieces of broken seed coat, but the bulk consists of undeveloped seeds ranging from what appear to be abortive ovules with a tiny halo of lint or fuzz to seeds as large as normal but flat and empty, and containing either no embryo or the remains of it. The seed coat of some of these is horny, but of the majority, brittle; all but the few normal seeds are light and cling to the lint with great tenacity.

*Chaff.* This consists of broken leaf, stalk, bract, with a varying proportion of small seed coat fragments.

*Dirt.* This contains finer particles of all constituents, but with a larger proportion of mineral matter together with fungal debris (if any).

*Dust.* This actually escapes in the fractionating process, but when collected and examined is found to consist of microscopic fragments of all constituents mentioned.

The mean percentage composition of the trash from the Blowing-room and Taker-in for 22 mills was found to be :—

|       |     |     |
|-------|-----|-----|
| Seed  | ... | 19% |
| Chaff | ... | 58% |
| Dirt  | ... | 19% |
| Dust  | ... | 4%  |

It is interesting to compare this with the trash constituents of the finished cloths. Just as in the case of neppy cloths, cloths which show an undue proportion of leaf and mote are periodically submitted for examination at the Institute. The following gives the incidence of the various types in about 20 cloths examined :—

|                   |     |     |
|-------------------|-----|-----|
| Fuzz motes        | ... | 88% |
| Dead cotton motes |     | 7%  |
| Stalk and leaf    | ... | 3%  |
| Foreign fibres    | ... | 2%  |

so that although the chaff, which consists mostly of stalk and leaf, forms such a large proportion of the trash in the raw cotton, practically the only survivor in the majority of finished cloths on which complaints of dirtiness are made is the tiny seed coat fragment with fuzz or lint attached, usually called bearded mote. Its survival is due, of course, to the clinging power of the lint or fuzz with which it is covered. Some of the card slivers from the mill tests were examined and practically the whole of the visible trash consisted of bearded motes.

A striking illustration of the surviving capacity of this type of trash was brought to our notice a few years ago by a mill which was spinning both Sudan American and Egyptian Uppers cotton. It was found that the Sudan American cotton was giving a much dirtier roving than the Egyptian Uppers, although there was no difference in the appearance of the raw cottons to account for this.

An examination of the trash in both rovings was made and in both it consisted entirely of these bearded motes, but there were far more present in the Sudan American sample than in the Uppers.

Fractionation of the raw cotton gave the following results :

|                | <i>Lint.</i> | <i>Seeds.</i> | <i>Chaff.</i> | <i>Dirt.</i> | <i>Dust.</i> |
|----------------|--------------|---------------|---------------|--------------|--------------|
| Uppers         | 97.3         | 1.3           | 0.8           | 0.4          | 0.2          |
| Sudan American | 96.9         | 0.8           | 1.1           | 1.2          | 0.06         |

so that there was little difference in either the total amount of trash or in the percentage amount of the various fractions to account for this difference.



## IMPURITIES IN COTTON—MOTES AND NEPS.

An examination of the nature of the chaff gave the solution of the problem:—whereas the Uppers chaff was composed almost entirely of pieces of leaf and stalk, the Sudan American chaff consisted of bearded motes of varying sizes, a large proportion of which must have survived to the roving, the leaf and stalk of the Uppers having been removed.

The question we should like to put to the grower is—How and where do these motes arise? Are they due to the breaking up and crushing of the immature seeds in ginning and processing, or to gin cutting of normally developed seeds? Is this another immaturity problem or is the ginnery at fault?

This could be decided immediately if it were possible to distinguish between broken up undeveloped seeds and broken up normal seeds. Unfortunately this has not proved possible, because at least 60 per cent. of the undeveloped seeds taken from the four main types of cotton investigated (American, Indian, Egyptian, and Peruvian) have normally thickened fuzz and lint, the latter being very short, so that microscopic examination gives us no reliable evidence of the origin of the motes.

However, it has proved possible to throw some light on this question by comparing the amounts of bearded motes and seeds (where they were entirely undeveloped) from the raw cotton (i.e., after ginning) with those from the taker-in droppings of the card: the taker-in, which is the most powerful agent in the spinning processes for removing such material, acts at the end of the opening and cleaning processes in spinning. If any crushing had taken place in these processes, therefore, the ratio of bearded motes to seed would obviously increase as a consequence. In this comparison the following average results were obtained for the four main types of cotton mentioned above.

|                           | <i>Bearded Motes<br/>from chaff fraction.</i> | <i>Total Seed<br/>fraction.</i> | <i>Ratio<br/>Bearded motes/seed.</i> |
|---------------------------|-----------------------------------------------|---------------------------------|--------------------------------------|
| Raw Cotton ... ..         | 0.61 %                                        | 0.69 %                          | 0.89                                 |
| Taker-in Droppings ... .. | 11.1 %                                        | 14.2 %                          | 0.79                                 |

The proportion of bearded mote to seed is slightly less in the taker-in fraction than in the raw cotton so that this affords no evidence of a crushing of the seeds to form the bearded motes in the opening and cleaning processes of spinning.

It was found, however, that the trash from the Peruvian cotton alone contained an average amount of undeveloped seed, but no bearded mote in its chaff. On examining the chaff fraction of the taker-in

trash it was found that 12.7 per cent. of this consisted of bearded motes which must have been formed from the breaking up of the undeveloped seed in the blowroom.

The following observations have been recorded in Tech. Bull. 396 U.S. Dept. of Agriculture by Miss Norma L. Pearson. Of 39,000 ginned seeds examined, 76 only were cut by the saw gin, and 1,602 showed a clipping at the chalazal end (this portion of the seed is said to be very brittle). Since the number of gin-cut normal seeds is almost negligible, and the chalazal clippings very minute compared with the bearded motes found in the chaff, it would appear that the majority of seed coat fragments are broken undeveloped seeds, some of which are crushed in the gin, and others pass through uninjured to be broken up in the opening machinery and the carding engine, and thus provide the surviving trash fraction in the yarns.

Thus we have two further problems to offer the grower, in addition to the general immaturity problem—the investigation of the origin of those two troublesome impurities, bearded motes and homogeneous dead cotton neps, both of which, though they may not affect the intrinsic spinning value of the cotton survive through to the finished cloth, and lessen its appeal to that most important person—the buyer.

## IMPURITIES IN COTTON—MOTES AND NEPS.

FIG. 1. RELATION BETWEEN MATURITY RATIO AND ORDER OF NEPPINESS.

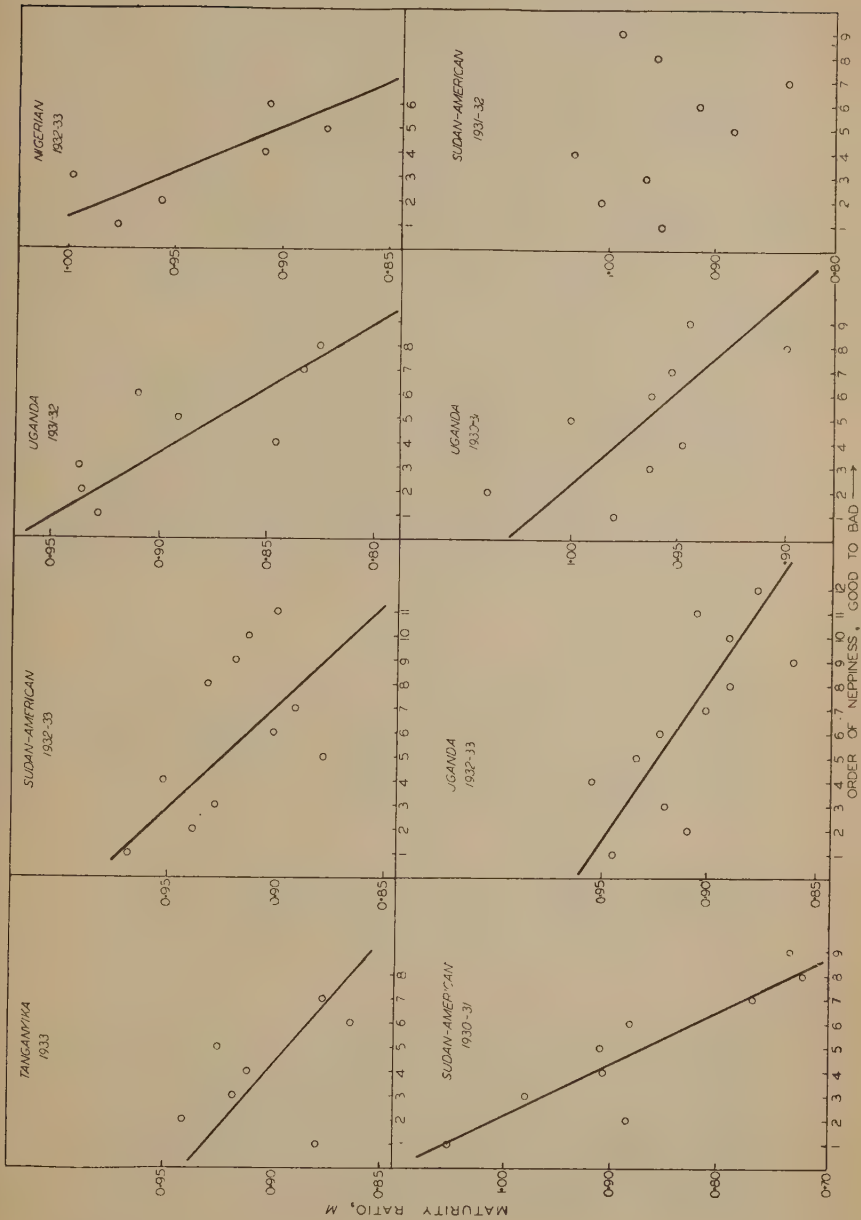


FIG. 2. RELATION BETWEEN MATURITY RATIO AND NUMBER OF COTTON NEPS PER CARD.

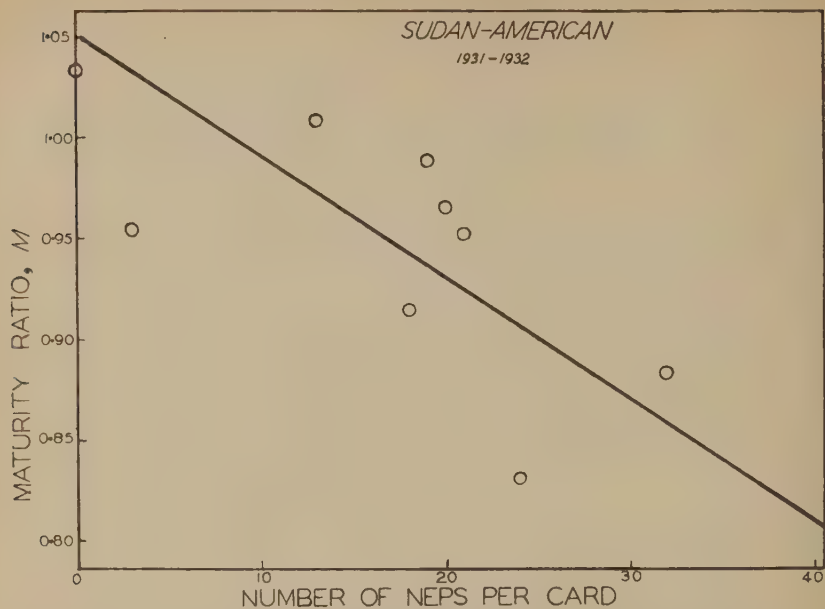
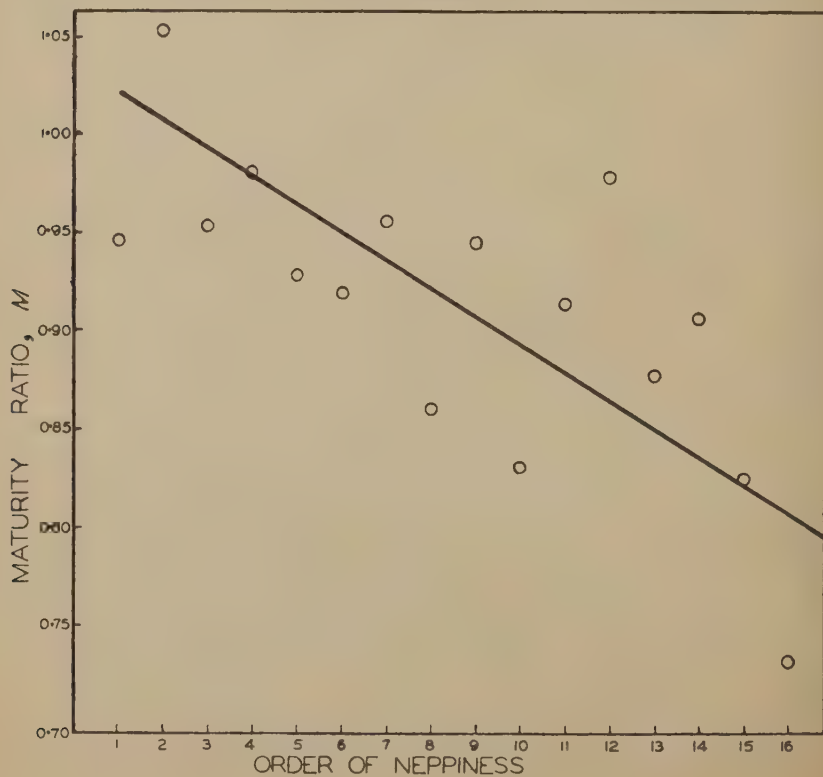


FIG. 3. TWO COTTONS FROM EACH OF PRECEDING EIGHT SETS ARRANGED IN ORDER OF NEPPINESS.



## IMPURITIES IN COTTON—MOTES AND NEPS.

In introducing the paper, MISS CLEGG said that since the general question of immaturity of raw cotton had been raised at the Conference in 1930, the relation of immaturity to the appearance of yarns and finished cloths had been considered. A classification of neps from the finished cloths had been made, which to some extent distinguished between those pre-existent in the raw cotton and those formed in processing. The general conclusion that had been reached was that there was a definite correlation between the maturity ratio of the raw cotton and the neppiness of the yarns and cloths, but that the most troublesome nep in dyed and finished cloths was the homogenous dead nep, *i.e.* the type pre-existent in the raw cotton.

The second troublesome impurity was the bearded mote—a tiny speck of seed coat with fuzz or lint attached. On examination of the waste removed during the processing of the raw cotton, it was concluded that those motes were formed by the breaking and grinding up of the immature seeds present in varying quantities in the raw cotton. Since it appeared to be an almost impossible task for the spinner to remove those neps and the motes, the problem was put to the grower to discover their origin and any possible means of preventing them.

## DISCUSSION.

MR. TROUGHT said that work in America and the Punjab tended to show that the incidence in seed cotton of those "motes" which arose from abortive ovules was due, in part, to the genetic constitution of certain cottons, and in part, to cultural and climatic conditions. A reduction in their number should therefore be possible by altering the variety or the cultural conditions.

*In the Sudan, in some types of cotton, the adherence of the lint to the seed coat was greater than in other types. Bits of seed coat were therefore more liable to be pulled away in ginning and thus get into the ginned lint. He used that character in the later stages of selection as a reason for discarding.*

DR. AHMAD said that at the Matunga Technical Institute, Bombay, they had been working on the relationship between hair maturity and neps. About 40 Indian cottons of various types had been tested, which had been shown to give a fairly wide range of neps in the yarns. Immaturity counts had shown a fairly high significant correlation coefficient between the number of immature hairs and the number of neps in the yarn. The work was being continued. He could confirm the experience of Mr. Trought as to the effect of cultural methods and growing



conditions on the percentage of immature hairs. Experiments on Punjab-American cottons had shown that the dates on which they had been irrigated and the amount of water supplied had a very pronounced effect on the immaturity count of these cottons.

MR. HOSKING said that he had found in practice that the undeveloped ovules very often got through into the lint in ginning. They were certainly to a great extent hereditary.

*The first type of neps in Miss Clegg's classification was due chiefly to the cotton itself. The immature and mixed neps might be caused by insect damage, but they might also be due to a specific character of the cotton in question.*

*The question of broken seed was one which had come to the fore very much in Uganda. Last season they had very dry weather while the crop was maturing and at the time of the early ginning. At numbers of ginneries he had seen vast quantities of broken seed being picked out of the gins. Only the actual insides of the seed were picked out, much of the seed coat going through with the ginned lint. Some of this would no doubt come out in the processing, but some would not. When rain came a little later, the breakage of the seed decreased markedly.*

*He would be glad to know Miss Clegg's views as to the cause of the dead neps.*

MISS CLEGG replied that the homogeneous dead neps appeared in the raw cotton, and seemed to consist of a tuft of about fifty to a hundred hairs, all about the same age and with practically no secondary thickening at all. Their origin was not known: it was recognised, however, that the general immaturity of the crop was often due to climatic conditions during growth, or to some physiological condition of the plant. Some of those homogeneous dead neps might be due either to some condition of the plant which caused the production of hairs which had no secondary thickening at all, starvation of some of the seeds, and development of only, say, 50 per cent. of the seeds in the boll, or it might be due to attack on the boll. The question she asked the growers was—was the cause climatic, was it something inherent in the plant, or was it mycological?

DR. BALLS said that the answer to Miss Clegg's final question could only be that the grower could not prevent such occurrences; they were inherent in the fact that the cotton plant was a growing plant. He could to some extent alter their incidence, as had already been said, by looking for circumstances and varieties where they were less common, but they were inevitable. He thought that those working on the industrial side were apt to overlook the fact that with cotton the bolls developed week after week, in some cases for three months, and every one of those bolls passed through a different environmental history.

*A boll that set on one day passed through a different environment from one that set the next day. Moreover, one should bear in mind that inside every single boll there was the variation in the life history of the individual hairs, roughly 10,000 in number, on the single seed. There were, say, 15 seeds in the boll, with 10,000 hairs on each seed, and every hair was an individual passing through a different individual history, even though they were all inside the same boll. The supply of food material to any one hair must necessarily be different from that to another one.*

*One thing which he would suggest to Miss Clegg was that, as her data accumulated, she should classify them by locality and so forth, and be able to point out to the growers that certain types of trouble occurred more commonly from certain districts. That would be of direct assistance in making it possible to say, for example, that a certain pest which occurred in one country was responsible for increasing the incidence of a certain type of nep trouble.*

DR. PICKARD said that all present would doubtless agree with what Dr. Balls had said as regards the impossibility of preventing the occurrence of hairs with little or no secondary thickening, but it provided small comfort to the dyer, to whom these neps gave serious trouble.

DR. PEIRCE agreed that the difficulties described must occur to some extent ; it was the extent which made all the difference. Possibly some simple discrimination in picking might have a big effect on the incidence. He had noticed in bales that those bits of immature cotton and ovules both occurred in patches.

MR. PARNELL said that he was grateful to Dr. Balls for the remarks that he had made in the course of the discussion. In his experience also there was no means of securing uniformity in the hairs. He mentioned also the effect of adverse growing conditions on the quality of the lint. He instanced strains which differed considerably from each other in the proportion of bolls shed owing to such adverse conditions : one might shed a great part of its bolls and produce nothing until conditions improved ; the bolls then formed contained comparatively good lint, whereas the lint from types which retained their bolls through bad conditions was definitely impaired. It was possible to produce one strain which would give better lint under adverse conditions than another, but it was at the expense of yield.

*He was able to confirm what Mr. Trought and Mr. Hosking had said that the amount of undeveloped seed was partly hereditary. There was also, as Mr. Trought had said, a great difference between varieties in the ease with which the lint parted from the seed. In strains in which a good hard pull was necessary to separate even a small tuft of fibre from the seed, there was a great tendency for part of the seed coat to come away. In some cases, they had decided to discard a strain almost on that ground alone.*

- MR. BARRITT said that Dr. Harland had stated a few years ago that the formation of undeveloped ovules was hereditary and was due to some lethal factor inherited by the particular ovule.
- DR. BALLS replied that Dr. Harland had pointed out that there were such lethal factors in certain hybrids, and where such factors existed some ovules in the boll were empty, but empty ovules could also occur sporadically from accidental causes.
- DR. PICKARD said that the question of specks in a dyed cloth presented a serious problem because no facilities at present existed on a sufficiently large scale for enumerating the proportions of the various types of nep described by Miss Clegg in the yarn, grey cloth and the dyed cloth respectively. These specks would not take the dye, or took it abnormally, and the problem seemed to go right back to the raw cotton in the field. It was encouraging to hear from various speakers that the incidence of some of these specks due probably to undeveloped seed might be reduced.
- MR. SLATER said that he could not quite share Miss Clegg's view that the spinner was helpless in eradicating neps. They could be removed by adequate combing, but, in virtue of the present low yarn prices, it was not profitable to ensure complete removal. Waste losses must be drastically kept down, and extra combing naturally led to increased waste. The larger pieces of seed coat, stalk and leaf were giving the spinner considerable trouble, especially in the recent developments in carding practice. The spinner would welcome any reduction in the quantity of nep-forming immature fibre and the larger pieces of trash.
- MISS CLEGG replied that most of the yarns received at the Shirley Institute were not combed.
- MR. SLATER suggested that the point should be stressed that what had been said regarding neps referred mainly to carded yarns.
- MR. MARCH suggested that more emphasis should be laid on insect damage. Seeds damaged in this way were in his opinion the source of a considerable proportion of the motes.
- PROFESSOR ROBERTS asked whether they had any evidence at the Shirley Institute as to the difference in cleanliness in cotton produced by the use of saw gins and roller gins respectively, and, if not, whether such a test could be undertaken.

The reply was given that such work could be undertaken, but that up to the present samples of saw ginned and roller ginned cotton had only been examined from the point of view of neppiness and not of cleanliness. The saw gin tended to produce more nep, but possibly fewer motes.

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**A GENERAL REVIEW OF SPINNING TESTS ON EMPIRE COTTONS.**

BY

C. UNDERWOOD (*Shirley Institute*).

In this paper an outline is given of the general characteristics of Empire cottons, as revealed in spinning tests carried out under the supervision of the Shirley Institute; outstanding features are noted, and where possible comparisons are made with the standard commercial types with which Empire cottons compete. The results of tests on samples from successive seasons indicate the extent of the progress of selection work in the various countries, and the degree of success in eliminating various undesirable characteristics, such as neppiness. It must be borne in mind throughout that spinning tests on small samples—though invaluable as comparative tests—cannot be assumed to indicate completely the behaviour of the cotton when processed in bulk under mill conditions. For example, a tendency to “lick,” i.e. stick to the rollers during drafting, may appear trivial and unimportant in a spinning test, but in commercial processing might easily prove fatal to economic production. Broadly speaking, however, there is no doubt that the spinning test result is the best guide at present available to the spinning value of a cotton.

**PART I. GENERAL.**

Spinning tests have been carried out by the Shirley Institute since 1924, and Table I indicates the number of samples received from the various cotton-growing countries for the years 1924-34. Up to the end of 1930, the spinning of the samples was carried out in mills; since that time it has been done in the experimental workrooms at the Shirley Institute, a difference which must be remembered when comparing tests done before 1931 with those done since. An outline of the technique at present employed was published in the *Empire Cotton Growing Review*, Vol. IX, No. 4.

TABLE SHOWING NUMBER OF SAMPLES SENT FOR TEST FROM EACH COUNTRY DURING YEARS 1924-1934.

| Season.               | 1924 | 1925 | 1926 | 1927                       | 1928                      | 1929                       | 1930     | 1931                              | 1932                         | 1933     | Total for countries. |
|-----------------------|------|------|------|----------------------------|---------------------------|----------------------------|----------|-----------------------------------|------------------------------|----------|----------------------|
| Uganda ...            | 5    | —    | 2    | 4                          | 7                         | 18                         | 23       | 19                                | 25                           | 20       | 128                  |
| Sudan ...             | —    | —    | 8    | 65 { 30 Amer.<br>35 Sakel. | 23 { 15 Amer.<br>8 Sakel. | 29 { 19 Amer.<br>10 Sakel. | 22 Amer. | 29 { 22 Amer.<br>7 Sakel.<br>H.C. | 12 Amer.                     | 12 Amer. | 200                  |
| Nigeria ...           | —    | —    | 6    | 8 { 4 N.N.<br>4 S.N.       | 4                         | 12                         | 13       | 9 N.N.<br>20 S.N.<br>(H.C.)       | 14 N.N.<br>35 S.N.<br>(H.C.) | 11       | 132                  |
| Tanganyika ...        | —    | —    | 19   | 21                         | 12                        | 23—5 for H.C.<br>only.     | 1        | 27                                | 27                           | 13       | 143                  |
| South Africa ...      | —    | 2    | 8    | 7                          | —                         | 3                          | 9        | 7                                 | 8                            | 8        | 52                   |
| Queensland ...        | —    | 8    | —    | —                          | —                         | —                          | —        | —                                 | —                            | —        | 8                    |
| Nyasaland ...         | —    | —    | 6    | —                          | —                         | —                          | —        | —                                 | —                            | —        | 6                    |
| West Indies ...       | —    | 1    | —    | —                          | —                         | —                          | —        | —                                 | 8                            | —        | 9                    |
| Cyprus ...            | —    | —    | 7    | —                          | —                         | —                          | —        | —                                 | 4                            | —        | 11                   |
| Iraq ...              | —    | —    | —    | —                          | 9                         | —                          | —        | —                                 | —                            | —        | 9                    |
| Southern Rhodesia ... | —    | —    | —    | —                          | 5                         | 6                          | 5        | —                                 | 5                            | —        | 21                   |
| Gold Coast ...        | —    | —    | —    | —                          | —                         | —                          | —        | 1 H.C. only.                      | —                            | —        | 1                    |
| Fiji ...              | —    | —    | —    | 4                          | 2                         | 2                          | 8        | 4                                 | —                            | —        | 20                   |
| Ceylon ...            | —    | —    | —    | —                          | 2                         | 6                          | —        | —                                 | —                            | —        | 10                   |
| Northern Rhodesia ... | —    | —    | 2    | —                          | —                         | —                          | —        | —                                 | —                            | —        | 2                    |
| Sierra Leone ...      | 1    | —    | —    | —                          | —                         | —                          | —        | —                                 | —                            | —        | 1                    |
| New Guinea ...        | 1    | 1    | —    | —                          | —                         | —                          | —        | —                                 | —                            | —        | 2                    |
| Kenya ...             | —    | —    | 1    | —                          | —                         | —                          | —        | —                                 | —                            | —        | 1                    |
| Total for Years ...   | 11   | 12   | 59   | 109                        | 62                        | 99                         | 86       | 116                               | 138                          | 64       | 756                  |

N.N. = Northern Nigeria. S.N. = Southern Nigeria. H.C. = Hair Character Tests.



It will be seen that over 750 samples have been tested in the decade under review, the majority coming, as would be expected, from the major cotton-growing countries, viz. Uganda (128), Sudan (200), Nigeria (132) and Tanganyika (143). These four countries have sent samples regularly every year since 1926, and thus provide a most useful set of data by which to judge the progress of selection work, etc., in the countries concerned. (We shall return later to a discussion of these individual countries.) Next in order comes South Africa, samples having been received regularly from this country every year since 1925, with one exception (1928), giving a total of 52. Southern Rhodesia has contributed samples since 1928, up to a total of 21, and a few samples are usually received from Fiji, the total for this country since 1927 being 20. Of the other countries tabulated only Cyprus (11) and Ceylon (10) reach double figures, and samples from these places are very intermittent, Cyprus having sent 7 in 1926, but none again until 1932; whilst Ceylon forwarded 4 in 1924, but no more till 1929, and none since then.

The smaller cotton-growing countries naturally contribute only occasional samples, except in those cases where special problems have arisen, such as Fiji, or where potentialities are considerable, such as Southern Rhodesia.

#### DISCUSSION OF SEPARATE COUNTRIES.

I. *Uganda*.—The number of samples forwarded from Uganda has increased appreciably in recent years, owing, no doubt, to the establishment of the Bukalasa Experiment Station, and to the necessity for comparing S.G. 29 with N. 17 in various localities in the Eastern Province.

Uganda cottons are considered on the whole to be the best of the Empire American-type cottons. They frequently possess a staple length of  $1\frac{3}{16}$  in. and higher; the hair-weight per centimetre of a normally developed sample of good Uganda cotton is rarely greater than 160, and is often considerably lower than this figure, as for S.G. 23/8. They have an attractive feel, and a slightly creamy colour which, whilst a disadvantage in some markets, is very acceptable to spinners of Egyptian Uppers.

In common with practically all Empire cottons, Uganda cottons have sometimes suffered badly from immaturity and neppiness. The 1930 group was particularly bad in this respect, several of the samples of that year's crop having no more than 20 % normal hairs and as many as 50 % dead hairs. The 1932 samples were also somewhat immature and gave rather neppy yarns, whilst 1933 showed an improvement in this respect.

With regard to the selection work going on in Uganda, there is no doubt that great progress has been made—as far as spinning value is concerned—in the development of S.G. 23/8, a quite outstanding strain which has given consistently good results over a number of seasons.

II. *Sudan*.—The samples from the Sudan tested at the Shirley Institute included both Sakel and American types until 1929, since when the American-type cottons have been dealt with at the Shirley Institute, and the Sakel-type cottons at the Experimental Department of the Fine Cotton Spinners' and Doublers' Association, Bollington. Arrangements have recently been made, however, for both types to be tested at the Shirley Institute in future.

(a) *American-Type Samples*.—Tests have been made on numerous varieties grown under different conditions, e.g. irrigation, rainfall—ample and scanty, etc.—in comparison with Pump Scheme Strain, the established American variety of the Sudan. The outstanding features of the results are the consistent good appearance of the yarns spun from the Pump Scheme Strain samples, and the success in recent years of XA.1129, which has given yarns of exceptionally good strength and fairly good appearance, and of XA.129—a selection from Pump Scheme Strain—which spins yarns of good appearance although inferior in strength to those from XA.1129.

The comparison of the results of tests on samples grown under the different conditions strongly supports the conclusion mentioned by Bailey in his paper on “Nep in Sudan Cotton” submitted at the Corporation's Cotton-Growing Conference held at the Shirley Institute in 1930, viz. that, in the Sudan, neppiness is strongly associated with deficiency of water supply.

The best of the American-type samples—e.g. XA.1129 and XA.129—are definitely superior in spinning quality to either Memphis or Uppers, whilst Pump Scheme Strain has in recent years proved about equal to Memphis in spinning quality.

(b) *Sakel-Type Samples*.—The first set of Sakel-type samples, received in 1928, included 19 samples from a “date of sowing and picking” experiment. These particular samples were considered somewhat neppy and not up to the standard of good Sakel. A series of “variety test” samples gave decidedly better results, and in this and the succeeding seasons most of the strains tested compared favourably with Egyptian Sakel. (Samples of the 1929-33 seasons were tested by the Fine Cotton Spinners' Association.) A sample of a recently developed promising strain—X.1530—was spun last year at

the Shirley Institute, and gave results practically equal to those obtained from a good commercial Egyptian Sakel, although not so good as the best of the new Egyptian varieties.

It is thus clear that the Sudan is capable of producing medium-staple cottons of high quality, and Sakel cottons equal to good Egyptian Sakels, although experience has shown that there is a tendency to neppiness, particularly in some of the American varieties when the growing conditions are not entirely favourable.

### III. *Nigeria.*

(a) *Northern Nigeria.*—A comparison of the results obtained in recent years in spinning tests on Northern Nigerian cottons with those of the earliest tests in 1926 shows a more pronounced improvement in spinning quality than in the case of any other Empire cotton-growing country. The first samples—from the 1925-26 crop—were very irregular in staple, immature, and spun weak and neppy yarns; they were not considered suitable for counts finer than 20's. Recent tests on samples of Strain L—one of the best selections from the point of view of spinning quality—show that this variety, when grown under suitable conditions, is capable of spinning a good 60's twist, whilst even the ordinary B.C.G.A. Allen—the commercially grown variety—proved suitable for at least 50's. All the samples tested last year were superior to a Mississippi control, and several—including Strain L and Strain D—were better than a Memphis, also used as control. Whilst a tendency to neppiness is still apparent in occasional samples, it is quite certain that this defect has been markedly reduced in the case of Northern Nigerian cottons.

(b) *Southern Nigeria.*—The first spinning tests on samples of Ishan type were made in 1927, and the results showed that these cottons were similar to Peruvian in hair characters and spinning value, being of good staple length, but having a fairly high hair-weight per centimetre, and consequently spinning rather weak yarn compared with American of similar length. Between 1927 and 1931 a considerable deterioration occurred in the spinning quality of Ishan cotton, for a sample spun in 1931 gave very inferior results, being barely satisfactory in 20's counts and only slightly better than the native Meko. Spinners were said to be rejecting this cotton on the ground of "roughness," and an examination of the hair characters of a number of Ishan samples showed that roughness was probably closely associated with hair-weight per centimetre, the comparative failure in spinning quality of the later samples being attributed to the large increase in hair-weight per centimetre from 200 in 1927 to about 300 in 1931. Selections were then made for "smoothness" and a further series of samples examined in

1933 gave, in general, considerably lower values for hair-weight per centimetre, many of the samples giving values about 200, whilst a few fell as low as 170 and 180, showing that the selection work was proceeding in the right direction. Some of the samples with the lowest hair-weights were found to suffer from immaturity, which experience has shown to be undesirable, and attention should be concentrated rather on those samples giving a fairly low hair-weight in conjunction with normal wall-thickening.

#### IV. *Tanganyika.*

Some of the earliest cottons received from Tanganyika for spinning tests suffered badly from stain, and they were rather immature and somewhat neppy. Staining has been less frequent in samples tested more recently, but the tendency to neppiness is still present, though not to the same extent, in many Tanganyika cottons. Reports from the mills where the samples were spun until 1930, indicated that Tanganyika cottons were on the whole superior in strength to Memphis and Uppers cottons, but not so good in appearance. Cottons tested at the Shirley Institute have given similar results, except that U.4 and other South African strains have generally been decidedly weaker than the "local" varieties.

#### V. *South Africa.*

Since 1927, the samples received from South Africa have all been U.4 or selections from U.4. The spinning quality of the samples has varied little, the best being good in 50's and comparing favourably with a Mississippi used as control. They are, however, inferior in strength on the whole to East African cottons, although they have an advantage over the latter in being generally free from nep. This feature, together with their attractive colour and appearance, renders them acceptable to spinners of medium count yarns in which particularly high strength is not demanded.

#### VI. *Queensland.*

The eight samples of Queensland cotton tested in 1925 consisted of annual and ratooned samples of Acala and Durango cotton. On the whole, the annual samples were slightly superior in spinning quality to the ratooned, but the differences were not great and the data were too few to enable any general conclusions to be drawn. The cotton was of American type suitable for counts from 30's to 40's.

#### VII. *Nyasaland.*

Six samples from Nyasaland in 1926 gave satisfactory results in 44's, and were similar in character to American Upland.

VIII. *West Indies.*

A sample of Marie Galante cotton was tested in 1925, but no further samples were received from the West Indies until 1932, when samples of (1) long staple American Upland, suitable for counts up to 100's; (2) Trinidad Red Leaf, a medium staple cotton suitable for count's from 30's to 40's; (3) a mixture of V.13 and V.46, for hair characters only; (4) St. Vincent Commercial Superfine; and (5) Montserrat Sea Island, were tested. In the case of the St. Vincent and Montserrat samples a comparison was made between samples from which all defective seeds had been eliminated and those from which no such selection had been made. The conclusions reached were that the differences in spinning quality were not sufficiently great to justify the expense of the selection.

IX. *Cyprus.*

Seven samples tested in 1926 were all of the medium staple American type (Webber, Hartsville, Sunbeam, etc.) suitable for counts from 20's to 40's, and about equal in spinning value to a Commercial Texas. The 1932 samples consisted of two short-staple varieties—Lapithos and Titsiros—suitable for counts about 20's, together with samples of Mesowhite and U.4, which were much superior in staple and spinning value, being suitable for counts from 50's to 60's, and comparing favourably with a Memphis control.

X. *Iraq.*

The nine samples received from Iraq in 1928, gave excellent strengths in 42's, and were considered capable of at least 50's. They were superior in spinning quality to an Uppers control, but some of them gave trouble in processing owing to "licking" round the rollers on the draw-frame—a defect which was probably associated either with excessive wax on the hairs or with the presence of tiny particles of tissue from crushed seeds, such as have been found occasionally in Sudan, Egyptian, and Southern Rhodesian cottons. (See below under Section XI, Southern Rhodesia.)

XI. *Southern Rhodesia.*

Samples have been received fairly regularly from Southern Rhodesia. All except one—a sample of An. 12 tested in 1928—have been of U.4 origin and have given results very similar to those from the South African U.4 samples. The 1932 samples gave trouble with "licking," which was proved to be caused by tiny particles of adhesive matter, probably tissue from crushed seeds. Although this trouble does not affect the intrinsic spinning quality, it is, as mentioned in the introduction, very detrimental to the cotton from the point of view of commercial processing.



## XII. *Gold Coast.*

One sample consisting of a mixed lot was sent from the Gold Coast for hair character tests in 1931. It was found to be considerably inferior to the general run of West African Cotton, not being suitable for counts finer than about 30's. It was rather badly stained and somewhat immature.

## XIII. *Fiji.*

An interesting series of samples has been received from Fiji. In 1927 four selections from New Guinea Kidney cotton were tested; these were similar in character to Peruvian cotton, and were considered suitable for hosiery yarn of medium counts, in which high strength is not essential. In 1928, further selections from two of the 1927 samples were tested; again these fell in the Peruvian class, giving good-looking yarns of only moderate strength. The following season, two samples of Sea Island cotton were sent; these were, of course, entirely different in character from those previously tested, and were spun to 190's combed mule yarn; they gave yarns of good strength, but very neppy and much inferior in appearance to those spun from a West Indian Sea Island used as control. In 1931, eight Sea Island  $\times$  Kidney Hybrids were tested. These proved to be of Sea Island length, but with hair-weight per centimetre intermediate between Sea Island and Kidney cotton. Owing to this comparatively high hair-weight, they were found to be unsuitable for counts finer than 100's, and their market would probably be limited to the hosiery trade. The same limitation would apply to the four samples tested in 1933, as these were similar in their properties to the 1931 samples.

## XIV. *Ceylon.*

Four samples tested in 1924 proved to be fairly fine cottons of medium staple capable of competing with Uppers; but those sent in 1929 were much shorter and coarser, more closely resembling a short-stapled American in character, and suitable for counts from 20's to 40's.

## XV. *Northern Rhodesia.*

Two samples of Improved Bancroft and Arizona sent in 1926, compared very favourably with an American mixing of medium-stapled cottons.

## XVI. *Sierra Leone.*

The sample tested in 1924 was a fairly coarse, short-stapled cotton giving rather weak yarn in 29's.

XVII. *New Guinea.*

A sample of Durango cotton from the 1924 crop gave good results in counts from 40's to 50's, and compared favourably with Memphis, whilst a 1925 sample of Kidney cotton equalled Uppers in 54's doubling weft, but was weaker than Sakel in 60's twist.

XVIII. *Kenya.*

The only sample from Kenya—tested in 1926—proved to be very similar in hair characters and spinning value to the majority of East African cottons, giving yarns of satisfactory strength, but slightly neppy, in 38's count.

## CONCLUSION.

It is clear from this summary that there has been a definite improvement in the quality of the Empire cottons tested in the last ten years. As far as the spinner is concerned, neppiness and price seem to be the limiting factors in Empire cottons; on the whole, however, neppiness is less frequent than it was 10, or even 5, years ago in Empire cottons, this improvement being largely due, no doubt, to the abandonment of those strains showing pronounced tendency to this fault. Even so, a bad season may produce a crop of under-thickened cotton with a depressing effect on the reputation of Empire cottons in general. Though price may not be of primary interest to the cotton-breeder, it should be remembered that a new cotton will seldom, at first, command a price proportionate to its spinning value, simply because it is new, and therefore unknown to spinners. Markets should be studied in relation to the type of cotton developed. There still seems to be a good opening for a fairly short-stapled variety of the Texas type; complaints are frequently heard of the poor quality and neppiness of Texas cottons, and a good nep-free variety of about 1-in. to  $1\frac{1}{16}$ -in. staple would probably find a ready market. This is not to disparage the value of cottons like S.G. 23/8 in Uganda, Strain L in Nigeria, and XA.1129 in the Sudan, which will no doubt reap their just reward as soon as they become better known; and in this connection it is encouraging to be able to state, in conclusion, that the visitors to the twice-yearly exhibition held at the Shirley Institute show an increasing interest in the samples of Empire cotton on view, and in the results of the spinning tests made on these cottons.

MR. UNDERWOOD in introducing his paper, dealt first with the question raised by Mr. Parnell in the discussion on an earlier paper as to the type of sample sent for spinning tests. He stressed the difference

almost invariably encountered between purely spinning test samples and commercial samples of the same type of cotton, and pointed out that cotton breeders, as well as spinners, should always remember that commercially grown cotton was almost invariably inferior in spinning quality to specially grown experimental samples sent for spinning tests. Nevertheless, he emphasised the need for testing such experimental samples, since the primary object of the work was the development of superior varieties.

Turning to the paper itself, Mr. Underwood drew attention to the interesting case of Ishan cotton, the spinning value of which had deteriorated very much between the years 1927 and 1931. Mr. Lewin, formerly of Southern Nigeria, had suggested to him in conversation that this apparent deterioration was due to the fact that the 1931 cotton was a bulk sample of commercial Ishan, whereas the 1927 samples were specially grown spinning test samples. The speaker felt, however, that this explanation was by no means sufficient to account for the very large difference in spinning quality, the 1927 samples being suitable for counts from 40's to 50's whilst the 1931 sample was barely satisfactory in 20's. Perhaps some other cotton breeders would like to express an opinion as to whether such a big drop in quality could reasonably be attributed to the cause Mr. Lewin had suggested.

Mr. Underwood then drew attention to the defect found in some samples of Iraq and Southern Rhodesian cotton, viz., the tendency to stick to the rollers on the drawframe, and mentioned that this trouble had also occasionally occurred in samples of Sudan Sakel and Egyptian cotton.

After pointing out the interesting results of the Sea Island  $\times$  Kidney crosses, in which the hybrids were found to possess the staple length of Sea Island with a hair weight very much coarser than normal Sea Island, thus rendering them unsuitable for counts finer than 100's, Mr. Underwood referred to the long staple of the cottons usually cultivated in most Empire fields, and to the statement in his paper that "a good nep-free variety of about 1 inch to  $1\frac{1}{16}$  inch would probably find a ready market;" of course a much higher yield of such a cotton would be required to compensate the grower for the lower premium received on account of quality. He concluded by tentatively suggesting the need for some closer liaison between the marketing end of the cotton trade and the growing end, so that breeders could obtain more easily than at present sound information as to the types of cotton which would command a ready market.

## DISCUSSION.

MR. SLATER suggested that it was impossible to ascertain the probable value of new cottons unless during spinning tests on them a commercial sample, preferably one from the same district, was run through as a control at the same time.

DR. PICKARD pointed out that when the samples reached the Shirley Institute for the first time, they usually represented only a very small crop, and no commercial sample from the same district was available as a control in such cases. It was when the strain had been multiplied to a commercial scale that deterioration, as measured by the spinning test, became obvious.

MR. SLATER suggested that growers changed their samples too frequently. It was difficult to follow them up unless they sent the same samples for at least five years in succession.

DR. PICKARD agreed that, if facilities existed, spinning tests could be done more thoroughly on the same samples in successive years as the crop increased, but he regarded it as more important to be able to give information to the grower at the earliest possible moment as to the spinning quality of the cotton.

DR. BALLS said there were definitely two different points of view. To enable discrimination to be made between the spinning value of two cottons, the grower must send up most exactly comparable material for test, but at Giza for some years past they had tried to devise some way of "spoiling" the samples which were sent, doing this spoiling uniformly, so that it would not vitiate the comparison. They had seriously considered, for instance, the practicability of preparing samples by the addition of broken-up leaf and dirt, in order to prevent too good an opinion being formed on mere inspection, because when these pedigree cottons were grown in chequer-board plots by the normal straight cultivation of the district, but were necessarily picked with rather more care and looked nice and clean, the samples appeared too good and that created a prejudice in their favour. The difficulty was to find a technique that would create a prejudice against them and yet not vitiate the comparison.

*In that connection, the point that Mr. Underwood had raised was important. When a variety got into the hands of small growers, necessarily the average of their produce was inferior to that of the straight cultivation on a good estate which it got in its early trial stages, and some way was required of preventing too good an opinion being formed on it at the beginning.*

*Mr. Underwood's other point about short staple was also of great importance. In Egypt the bread and cheese cotton was the Upper, which was always saleable. They were content to sell it at a very small premium over American, and Egypt could be emptied of every bale of Uppers which it contained before any American was sold at all. Even during the depths of the depression, it had found a ready market.*

*Dr. Balls thought there was a future, however—and it was a future which they were actively exploring at Giza—for breeding fine short staple cotton. A certain measure of success had already been achieved, and a nice practical point had arisen. When the grader was handed a fine crop, the hair weight of which had been lowered from 19, the normal, to 17 in the new strain, and of which the spinning strength had shown a corresponding improvement, his comment was that he did not like it because there was no body in it. It would take several years to educate the market to understand that it was better than the original crop for certain uses.*

MR. BARRITT mentioned an article he had written some four years ago in the *Empire Cotton Growing Review*, pointing out that long staple cottons showed least variation in diameter, whilst short staple cottons showed least variation in length.

*Cotton breeders had always selected for length of staple, fineness being incidental to that factor. Selection for fineness amongst short staple cottons would give the spinner what he was always asking for, but never got, viz., uniformity of fibre properties. It would also create a new market. It was interesting to hear that such a cotton was now being sought in Egypt. It was the only way to improve on Uppers cotton without competing with fine Sakel on the one hand and long staple American on the other.*

MR. HOSKING said that Mr. Underwood's remark as to there being a market for cotton of shorter staple than that usually grown in the Empire, was of interest to Uganda, because in the Eastern Province they might have come to the parting of the ways. In the past they had grown Nyasaland Upland cotton up to  $1\frac{3}{16}$  inch staple. It was fairly popular with certain spinners, but its yield was not very good, it suffered from blackarm and was susceptible to jassid. They had selected from Mr. Parnell's original U.4 a cotton which they hoped could replace the Nyasaland Upland, but its staple was shorter. In a segregated area of 400 acres they were planting out selections from seed obtained from Mr. Parnell; the selections had a lint length of about  $1\frac{1}{8}$  inch. They wished to find out if there was a market for that cotton. It gave an increase in yield of 20 to 30 per cent. as compared with Nyasaland Upland, and would probably fetch 10 to 15 points less. If there was a market for it, the overheads in the ginneries would be reduced because of the bigger crop, and the revenue of the country would be increased.

*In Buganda the position was rather different. There, Mr. Nye was working on his original selection of S.G.23/8, which had proved to be the best spinning cotton Uganda had produced, but only one broker, he believed, had ever liked that cotton. It was rather short, but fine.*

*S.G.23/8 was originally evolved in the Eastern Province, and it reached the 40-acre stage of multiplication: it then went down to a heavy attack of jassid. They had not had enough jassid, in other*



words, in previous seasons. That illustrated a point made by Mr. Thomas in the discussion on a paper on the previous day. Jassid did exist on the Buganda side, but it was hoped that S.G.23/8 might nevertheless succeed there. It had been given up altogether in the Eastern Province.

MAJOR CAMERON referred to the statement in Mr. Underwood's paper to the effect that the 1932 samples from Southern Rhodesia gave trouble with "licking" round the rollers on the draw-frame. He asked whether this defect showed itself in samples sent from Southern Rhodesia in all years. There was a striking variation from year to year in the softness of Rhodesian cotton.

MR. UNDERWOOD replied that it was only the 1932 samples that had given rise to this difficulty.

MR. VOELCKER, in the absence of Mr. Lewin, mentioned the roughness that had been found in certain samples of Ishan cotton described in Mr. Underwood's paper. He explained that the original selections were made chiefly on yield, agricultural qualities, and resistance to disease. They were made, moreover, before the economic depression was felt, and the question of their roughness was doubtless overlooked by the Agricultural Department at that time. Breeding work was continued on the same lines, and when the depression was felt, the limited demand that existed for that type of cotton became even more restricted, and its roughness was probably one of the factors that told against it.

MR. SAMPSON mentioned that when he was on the Corporation's staff in Nyasaland, he expressed doubt as to whether better results would not be obtained in that country with a shorter stapled type of cotton, but his view was looked on askance at the time. The view he held was that a shorter cotton would probably be found to give a higher yield, and this was confirmed by a tour he made of the Cotton Belt of the United States, where he was told that for each additional one-sixteenth inch in staple there was a corresponding decrease in yield of about  $12\frac{1}{2}$  per cent. Moreover, certain States did not even attempt to grow a staple cotton, because they recognised that their climatic conditions were not suited to it.

As regards the deterioration that had been noted in Ishan cotton, Mr. Sampson suggested that possibly in the early stages the samples sent home by the Agricultural Department had been grown as a pure crop, whereas in later years, when the crop was grown as a part of native cultivation, the cotton was doubtless planted through yams and other crops, and that that factor had caused the deterioration in quality.

SIR WILLIAM HIMBURY said that in general he thought that the most paying type for each country was that which was found to be best suited to its soil and climate, but he uttered a word of warning against growing short-staple cotton, owing to the fact that it would encounter

*the keenest competition from America and from India, and he doubted whether most parts of the Empire could compete with the conditions in those countries as regards both labour and transport. Another point that he thought should be borne in mind was that the longer staple cottons at present grown fetch a considerable premium on Middling American, say 100 to 150 points on, and he had some doubts as to whether most producing countries in the Empire could profitably grow cotton which did not command that premium. If there were to be a change over to a shorter staple, the authorities would not only have to satisfy themselves as to the points that he had mentioned, namely, their ability to compete with such countries as America and India, and the absence of any premium on their cotton, but they would also have to make sure that when a lower valued cotton was introduced, the native grower would obtain a sufficiently increased yield to compensate him for the loss of the premium he was now receiving for the longer types of cotton.*

DR. BALLS said he fully agreed with Sir William Himbury, and in so far as he had advocated attention being paid to the cultivation of short cottons, he wished to emphasise that he attached no importance to shortness alone, except that it widened the market in that existing roller settings could be used. But a cotton which was fine as well as short was a more valuable product and had a wider market at its disposal. So far as shortness alone was concerned, however, he supported what Sir William Himbury had just said.

## THE RELATION BETWEEN FIBRE PROPERTIES AND SPINNING VALUE.

BY

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### INTRODUCTION.

The results of work on the relationship between the fibre properties and spinning value of Indian cottons have already been published and a memoir dealing with the same problem for Empire and Egyptian cottons is in preparation. The main problem is clearly of vital interest to cotton breeders, and it is suggested that further progress is unlikely without their active co-operation in supplying special material of the character indicated; in any case, it is hoped that the speculative nature of some of the suggestions made will not fail to stimulate a valuable discussion. In the present paper it is proposed merely to indicate the scope of the analyses, summarise the results, and discuss various interesting points arising out of the different conclusions reached in the three types of cotton.

## FIBRE PROPERTIES AND SPINNING VALUE.

## PART I.—CLASSIFICATION OF COTTONS INVESTIGATED.

(a) *Indian Cottons.* Table I has been drawn up so as to give some idea of the range of the 95 Indian cottons for which the analysis of spinning value has been completed. From Table I it will be clear that all classes in the counts range from 7's to 42's are well represented; there is also a good distribution of the samples in the various intervals of fibre-length between 0.66 inch and 1.01 inches. The relative range for hair-weight per centimetre is much greater than that for fibre-length, but about two-thirds of the samples fall within the two intervals represented by the range 152 to 240 ( $10^{-5}$  mgm.). These Indian cottons represent a considerable diversity of type, there being 39 of American type (*G. hirsutum*), and 56 of Asiatic type, comprising 24 *G. herbaceum*, 16 *G. Indicum*, and 16 *G. neglectum*. An important feature of this investigation of spinning value was the attempt to obtain considerable accuracy in the measurements of the various properties; this is indicated in Table I by the number of tests per sample for several properties.

TABLE I.  
INDIAN COTTONS.

| Highest standard counts. | Frequency. | Mean Fibre Length (inch). | Frequency. | Mean hair-weight per centimetre ( $10^{-5}$ mgm.) | Frequency. |
|--------------------------|------------|---------------------------|------------|---------------------------------------------------|------------|
| 7—12's                   | 14         | 0.66—0.71                 | 8          | 107—151                                           | 7          |
| 13—18's                  | 12         | 0.72—0.77                 | 11         | 152—195                                           | 24         |
| 19—24's                  | 15         | 0.78—0.83                 | 22         | 196—240                                           | 41         |
| 25—30's                  | 18         | 0.84—0.89                 | 29         | 241—285                                           | 13         |
| 31—36's                  | 26         | 0.90—0.95                 | 18         | 286—330                                           | 3          |
| 37—42's                  | 10         | 0.96—1.01                 | 7          | 331—374                                           | 7          |
| No. of tests per sample: |            |                           |            |                                                   |            |
| 300 lea                  |            | 4 Balls Sorter            |            | 4,000 fibres, weighed                             |            |
| 1,200 single thread      |            | 8 Baer Sorter             |            | in 8 to 28 sub-groups.                            |            |

(b) *Empire Cottons.* An analysis has been carried out at the Shirley Institute of the results of fibre and spinning tests made there on 206 samples of Empire cottons sent by the Empire Cotton Growing Corporation; the fibre tests included measurements of length, variability of length, hair-weight per centimetre, and immaturity. Table II shows the ranges of variation of these cottons in highest standard counts, effective length, and hair-weight per centimetre; it will be seen that the cottons have a good distribution in count and hair-weight, but a comparatively poor distribution in effective length, nearly two-thirds of the effective lengths falling in the single interval 34 to 39 thirty-seconds of an inch. All except 8 of the cottons are of American ancestry (*G. hirsutum*) and have been grown in various parts of Africa—chiefly Sudan, Nigeria, Uganda, Tanganyika, Southern Rhodesia,

and South Africa. In these respects, therefore, the Empire samples do not display the diversity that characterised the Indian cotton samples.

TABLE II.  
EMPIRE COTTONS.

| Highest standard counts. | Frequency. | Effective length ( $\frac{3}{8}$ "). | Frequency. | Mean hair-weight per centimetre ( $10^{-5}$ mgm.). | Frequency. |
|--------------------------|------------|--------------------------------------|------------|----------------------------------------------------|------------|
| 10—19's                  | 1          | 30—33                                | 13         | 90—119                                             | 8          |
| 20—39's                  | 36         | 34—39                                | 130        | 120—139                                            | 31         |
| 40—59's                  | 112        | 40—45                                | 55         | 140—159                                            | 59         |
| 60—79's                  | 42         | 46—51                                | 1          | 160—179                                            | 78         |
| 80—99's                  | 15         | 52—55                                | 7          | 180—199                                            | 30         |

No. of tests per sample, 32 lea; 2 Baer Sorter; { 2, each involving 500 hairs weighed in 5 sub-groups.

(c) *Egyptian Cottons*. 165 samples, chiefly Egyptian, have also been subjected to fibre and spinning tests at the Shirley Institute, and to a correlation analysis. The measured fibre properties were the same as for the Empire cottons (length, variability of length, hair-weight per centimetre, and maturity). Table III shows the ranges of variation of these samples in highest standard counts, effective length, and hair-weight per centimetre; it will be seen that there is a good distribution in all these properties. This is partly due to the non-Egyptian samples included in the series; whereas all the Egyptian samples are presumably of closely related ancestry (*G. peruvianum*) 31 of the non-Egyptian samples are of American type (*G. hirsutum*) and fall at one end of each frequency-distribution, whilst the remaining 6 are of Sea Island type (*G. barbadense*) and fall at the other end of each frequency-distribution.

TABLE III.  
CHIEFLY EGYPTIAN COTTONS.

| Highest standard counts. | Frequency. | Effective length ( $\frac{3}{8}$ "). | Frequency. | Mean hair-weight per centimetre ( $10^{-5}$ mgm.). | Frequency. |
|--------------------------|------------|--------------------------------------|------------|----------------------------------------------------|------------|
| 10—29's                  | 9          | 28—33                                | 9          | 100—109                                            | 4          |
| 30—49's                  | 18         | 34—37                                | 16         | 110—129                                            | 37         |
| 50—69's                  | 33         | 38—41                                | 26         | 130—149                                            | 50         |
| 70—89's                  | 39         | 42—45                                | 28         | 150—169                                            | 37         |
| 90—109's                 | 43         | 46—49                                | 53         | 170—189                                            | 17         |
| 110—129's                | 20         | 50—53                                | 28         | 190—209                                            | 15         |
| 130—159's                | 3          | 54—61                                | 5          | 210—229                                            | 5          |

No. of tests per sample, 32 lea; 2 Baer Sorter; { 2, each involving 500 hairs weighed in 5 sub-groups.

(d) *Empire and Egyptian Cottons taken together*. The 206 Empire and the 165 Egyptian samples have also been combined into a single series of 371 samples. The ranges of variation of these samples are

## FIBRE PROPERTIES AND SPINNING VALUE.

naturally more diversified than those for either set of samples, as will be seen from Table 4.

TABLE IV.  
EMPIRE AND EGYPTIAN COTTONS TAKEN TOGETHER.

| <i>Highest standard counts.</i> | <i>Frequency.</i> | <i>Effective length. (32").</i> | <i>Frequency.</i> | <i>Mean hair-weight per centimetre. (10<sup>-5</sup> mgm.).</i> | <i>Frequency.</i> |
|---------------------------------|-------------------|---------------------------------|-------------------|-----------------------------------------------------------------|-------------------|
| 10—29's                         | 19                | 28—33                           | 22                | 90—109                                                          | 5                 |
| 30—49's                         | 90                | 34—37                           | 94                | 110—129                                                         | 60                |
| 50—69's                         | 132               | 38—41                           | 120               | 130—149                                                         | 92                |
| 70—89's                         | 61                | 42—45                           | 41                | 150—169                                                         | 118               |
| 90—109's                        | 46                | 46—49                           | 53                | 170—189                                                         | 72                |
| 110—129's                       | 20                | 50—53                           | 34                | 190—209                                                         | 19                |
| 130—159's                       | 3                 | 54—61                           | 7                 | 210—229                                                         | 5                 |

No. of tests per sample, 32 lea; 2 Baer Sorter; { 2, each involving 500 hairs weighed in 5 sub-groups.

## PART II.—SUMMARY OF RESULTS.

Perhaps the simplest method of comparing the outstanding conclusions drawn from the analyses of the Indian, Empire, and Egyptian cottons, is by means of the correlation coefficients between spinning quality (represented by the highest standard counts) and the several fibre-properties. The total correlation coefficients show in each case which of the measured fibre-properties is the best single index of spinning quality; and the partial correlation coefficients indicate what is apparently the most important fibre-property for spinning quality after due account has been taken of the effect of the correlation of the other measured fibre-properties. The various correlation coefficients are given in Table V.

TABLE V.  
CORRELATIONS OF SPINNING VALUE WITH FIBRE-PROPERTIES.

| <i>Cottons.</i>              | <i>Total Correlation Coefficient between Spinning Value and—</i> |                          |                            |                          | <i>Partial Correlation Coefficient between Spinning Value and—</i> |                            |                          |
|------------------------------|------------------------------------------------------------------|--------------------------|----------------------------|--------------------------|--------------------------------------------------------------------|----------------------------|--------------------------|
|                              | <i>Mean length.</i>                                              | <i>Effective length.</i> | <i>Hair-weight per cm.</i> | <i>Normal hairs (%).</i> | <i>Length.</i>                                                     | <i>Hair-weight per cm.</i> | <i>Normal hairs (%).</i> |
| Indian cottons:              |                                                                  |                          |                            |                          |                                                                    |                            |                          |
| 95 samples ...               | 0.87                                                             | —                        | —0.80                      | —                        | 0.70                                                               | —0.36                      | —                        |
| 460 samples ...              | 0.87                                                             | —                        | —0.75                      | —                        | —                                                                  | —                          | —                        |
| Empire cottons:              |                                                                  |                          |                            |                          |                                                                    |                            |                          |
| 206 samples ...              | 0.36                                                             | 0.38                     | —0.46                      | 0.12                     | 0.46                                                               | —0.74                      | 0.56                     |
| Egyptian cottons:            |                                                                  |                          |                            |                          |                                                                    |                            |                          |
| 165 samples ...              | 0.77                                                             | 0.82                     | —0.84                      | 0.42                     | 0.24                                                               | —0.65                      | 0.53                     |
| Empire and Egyptian cottons: |                                                                  |                          |                            |                          |                                                                    |                            |                          |
| 371 samples ...              | 0.70                                                             | 0.77                     | —0.68                      | 0.48                     | 0.39                                                               | —0.74                      | 0.59                     |

It should be noted that the "length" partial correlations are with the "mean fibre-length" of the Indian samples and with the



“effective length” of the other samples. The partial correlations of the Indian samples were obtained after eliminating the effect of the correlations of all the measured fibre-properties—length, hair-weight, width, strength, convolutions, and rigidity—except the particular one under consideration (length or hair-weight in Table V); similarly the partial correlations of the Empire and or Egyptian samples were obtained after eliminating the effect of the correlations of the three measured fibre-properties—length, hair-weight, and percentage of normal hairs—except the particular one under consideration.

The correlation coefficients in Table V give some idea of the relative importance of the outstanding fibre-properties; the extent to which these account for the variation of spinning value is shown in Table VI.

TABLE VI.  
PERCENTAGE OF VARIANCE IN SPINNING VALUE ACCOUNTED FOR IN TERMS OF MEASURED  
FIBRE-PROPERTIES.

| <i>Cottons.</i>           | <i>Length<br/>only.</i> | <i>Hair-weight<br/>only.</i> | <i>Percentage<br/>of normal<br/>hairs only.</i> | <i>Length and<br/>hair-weight<br/>together.</i> | <i>All measured<br/>fibre-<br/>properties<br/>of Table V.</i> |
|---------------------------|-------------------------|------------------------------|-------------------------------------------------|-------------------------------------------------|---------------------------------------------------------------|
| Indian (95) ... ..        | 76                      | 64                           | —                                               | 84                                              | 86                                                            |
| Empire (206) ... ..       | 14                      | 21                           | 1                                               | 43                                              | 61                                                            |
| Egyptian (165) ... ..     | 67                      | 70                           | 18                                              | 76                                              | 83                                                            |
| Empire and Egyptian (371) | 59                      | 46                           | 23                                              | 73                                              | 81                                                            |

### PART III.—DISCUSSION OF RESULTS.

There are several interesting points of difference between the results given in Part II for the Indian, Empire, and Egyptian cottons. From Tables V and VI it is at once clear that for the Empire cottons none of the measured fibre-properties is of any use alone as an index of spinning value; but for the Indian and Egyptian cottons, either fibre-length or hair-weight gives a good indication of spinning value, fibre-length being rather the better criterion for the Indian cottons; for the Empire and Egyptian cottons taken together neither fibre-length nor hair-weight is as good a criterion of spinning value as for the Indian or Egyptian alone, but fibre-length is again rather better than hair-weight. In no case does the percentage of normal hairs, taken alone, provide a satisfactory measure of spinning value.

But when the effects of the different fibre-properties are, as far as possible, disentangled from one another, by means of the partial

correlation coefficients, the story is a very different one. Only for the Indian cottons does the fibre-length show any superiority over the hair-weight as a measure of spinning value, yet for these cottons its superiority is most pronounced. For the Empire and Egyptian cottons, taken either separately or together, the fibre-length appears of subsidiary importance both to the hair-weight and to the percentage of normal hairs; in these cases the hair-weight appears to be of outstanding importance.

The question arises whether any interpretation may be offered for these apparently conflicting results in terms of the physical structure of the fibres and the yarns, with reference to the main distinctions between the three groups of cottons. First, then, is any explanation possible for the varying importance that fibre-length assumes in the three groups of cottons? It may, of course, be that for the comparatively coarse Indian cottons length really is the most important fibre property. The predominance of fibre-length in this group is likely to be accentuated by the fact that it is not possible to set the drafting rollers of the spinning machinery so that the very shortest cottons can give the very best performance of which they would be capable if even smaller rollers were commercially practicable and available. On the other hand, the scale adopted for assigning the highest standard counts is itself favourable to the coarsest counts, which are spun from the shortest cottons, and therefore compensates wholly or partially for the practical difficulty of not being able to use the roller settings most appropriate for them. The low correlation of fibre-length with the spinning value of the Empire cottons is partly accounted for by the fact that the frequency distribution of fibre-length is comparatively unsatisfactory, but the low partial correlation coefficients for fibre-length for the Egyptian cottons must apparently be taken to mean that for these cottons fibre-length is of subsidiary importance, especially when compared with the hair-weight.

With regard to fineness and immaturity, it is evident that in one respect these two fibre-properties are closely related; for in any one strain, a higher percentage of normal hairs connotes a higher hair-weight per centimetre. From this point of view, the results for the correlation coefficients of hair-weight and percentage of normal hairs with spinning value, for the Empire and Egyptian cottons, are of special interest. As hair-weight is negatively correlated with spinning value, and higher percentage of normal hairs leads to a higher hair-weight, it might be expected that the total correlation between the percentage of normal hairs and spinning value would

also be *negative*. Further, a negative partial correlation might also be expected from such simple considerations as that for hairs of equal weight per centimetre the normal hairs may be more rigid than the collapsed, less fully-thickened hairs of more elliptical cross-section, which would therefore draft better and also pack together better in a yarn, with greater cohesion. In point of fact, however, the percentage of normal hairs is *positively* correlated with spinning value. So either the normal hairs are not more rigid or better-packing than the thin-walled hairs of the same weight per centimetre, or there must be some special reason to account for the inversion, *e.g.*, the normally developed hairs may be more free from abnormalities of structure, or they may have a higher intrinsic strength than immature hairs.\* *In order to test these speculations, material is desired consisting of a number of different strains, each of them grown with different degrees of wall-thickening, and we trust that we may enlist the cooperation of cotton breeders in supplying us with material of this kind.*

Although the degree of maturity, discussed above, greatly affects the fineness, as measured by the hair-weight per centimetre, it must be remembered that the latter depends also on the cell-diameter, which is of particular importance when different varieties are under consideration, as in the groups of Indian, Empire, and Egyptian cottons. The degree of maturity depends greatly on the environment; this may also affect the cell-diameter, though to a less extent. Evidently both characters also depend on the strain, *i.e.*, on variety factors. But it is possible that the cell-diameter depends more on the variety factors, and the wall-thickness more on the environment factors. The question therefore arises whether any explanation of the differences in the relations of the Empire and Egyptian cottons may be explained by reference to the relative effects of variety and environment. Is it possible that the results for the Empire cottons, which are almost entirely of American Upland origin, are due to their being grown under widely varying conditions of climate and soil? On the other hand, are the results for the Egyptian cottons due to varietal differences being much greater for these cottons, with far less variable conditions of growth? If this is so,

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\* The results described later and also those in another paper presented to this Conference (by Peirce and Lord) indicate that the degree of maturity of a variety is sometimes comparatively unimportant as regards spinning value, but even so, it still remains a question why a cotton composed of fully-thickened hairs of small diameter spins better than a cotton composed of thin-walled hairs of large diameter having the same length and weight per centimetre.

an explanation may be possible, for the fact that the correlations between the hair characters themselves differ widely for the Indian, Empire, and Egyptian cottons. For example, there is a high negative relation between length and hair-weight per centimetre for the Indian and Egyptian cottons, yet for the Empire cottons this coefficient is positive and barely significant; while the result for the Empire cottons may be no more than a reflection of sampling and experimental errors for cottons not differing very much in fibre-length. It is also possibly due to the environment effect being greater than the variety effect, as the environment effect might act in the same direction on length and on wall-thickening, thus tending to produce a positive correlation between length and hair-weight per centimetre. Such a positive correlation, amounting to 0.63, has in fact been found for the seasonal effect alone, with the variety effect eliminated in the analysis of co-variance given for 17 Indian cottons, each tested in four seasons (see Table VIII). Again the total correlation coefficient between hair-weight per centimetre and percentage of normal hairs is positive and high for the Empire cottons, negative and barely significant for the Egyptian cottons. If the environments differ so much for the Empire cottons that the differences in hair-weight per centimetre are due primarily to differences of wall thickening, a high correlation would result between hair-weight and percentage of normal hairs; and if the Egyptian cottons differ chiefly in variety and have much the same environments, the correlation between hair-weight per centimetre and percentage of normal hairs should be comparatively small.

Of course, any difference in environment must be largely a matter of degree. Balls has given examples of the differential reaction of two varieties according as they are grown in Lower and Upper Egypt. Similar examples are no doubt to be brought forward from all large cotton-growing areas. Thus in India it is well known that the Punjab Americans, which do so well in the Punjab, are not nearly so suited for the cotton-growing tract of Central India. Similarly, Cambodia gives an excellent lint in Madras, but in Central India and in Burma the plant does not do so well and the lint is much inferior. In these examples not only is there a difference in the fibre-properties, but this is reflected in the spinning values. But the environment effect is not always equal in its incidence both on the fibre properties and on the spinning value. Exceptional examples of this are provided by the Dholleras cotton, Wagad 8, and the Punjab American 289F, for the

years 1926-27 and 1927-28; these cottons gave results as follows for the various fibre properties and spinning value.

| <i>Cotton.</i>           | <i>Season.</i> | <i>Mean<br/>hair-weight<br/>per cm.<br/>(10<sup>-6</sup> mgm.).</i> | <i>Mean<br/>fibre-length<br/>(inch).</i> | <i>Mean<br/>fibre-strength<br/>(oz.).</i> | <i>Highest<br/>standard<br/>count.</i> |
|--------------------------|----------------|---------------------------------------------------------------------|------------------------------------------|-------------------------------------------|----------------------------------------|
| Wagad 8 ...              | 1926-27        | 294                                                                 | 0.80                                     | 0.178                                     | 14                                     |
| Wagad 8 ...              | 1927-28        | 211                                                                 | 0.77 <sub>6</sub>                        | 0.124                                     | 15                                     |
| Punjab-<br>American 289F | 1926-27        | 109                                                                 | 0.97                                     | 0.109                                     | 40                                     |
| Punjab-<br>American 289F | 1927-28        | 158                                                                 | 0.94                                     | 0.135                                     | 42                                     |

It will be seen that these two cottons have been greatly affected in hair-weight and also in fibre strength by the difference in the conditions in the two seasons, and the difference in fibre-length has been comparatively small; yet there has been hardly any change in the spinning value. It therefore seems possible that where the change of environment is not very radical, but rather a seasonal or climatic variation, profound changes may be caused in the hair-weight without much change of the spinning value. But even this conclusion must not be pushed too far because in other seasons the Punjab American 289F has shown more variation in spinning value with less variation in hair-weight or fibre-length, possibly owing to differences in cell-diameter in those seasons.

So far as the effects of environment and variety in general are concerned, some evidence is available from the results of the tests on the 95 samples of Indian cotton previously referred to. Of these 95 samples, 68 represent 17 different varieties tested in each of the four seasons from 1926 to 1930. The properties of spinning value, fibre-length, and hair-weight are shown in Table VII for the different cottons in the four seasons, 1926-30. These values have been subjected to an analysis of variance and co-variance in order to determine the correlations due to differences of variety with the effect of season eliminated, and the correlations due to differences of season with the effect of variety eliminated; the results of the analysis are shown in Table VIII.



## FIBRE PROPERTIES AND SPINNING VALUE.

TABLE VII.  
COUNTS, FIBRE-LENGTH, AND HAIR-WEIGHT OF 17 INDIAN VARIETIES IN FOUR SEASONS.

| Property.                                                | Season. | Dharwar<br>1. | Gadag<br>1. | Surat<br>1027<br>A.L.F. | Wagad<br>8. | P.A.<br>4F. | P.A.<br>285F. | P.A.<br>289F. | Melli-<br>santi. | Ali-<br>garh<br>A.19. | K 22. | J.N. 1 | C.A. 9. | Umri<br>Bani. | Cam-<br>bodia<br>Co. 1. | Nan-<br>dial<br>14. | Hag-<br>ari<br>25. | Karun-<br>digan<br>C.7. |
|----------------------------------------------------------|---------|---------------|-------------|-------------------------|-------------|-------------|---------------|---------------|------------------|-----------------------|-------|--------|---------|---------------|-------------------------|---------------------|--------------------|-------------------------|
| Counts                                                   | 1926-27 | 34            | 38          | 32                      | 14          | 24          | 34            | 38            | 8                | 7                     | 12    | 14     | 34      | 24            | 33                      | 34                  | 26                 | 24                      |
|                                                          | 1927-28 | 32            | 38          | 30                      | 15          | 22          | 34            | 40            | 8                | 7                     | 12    | 14     | 30      | 24            | 34                      | 31                  | 26                 | 24                      |
|                                                          | 1928-29 | 34            | 26          | 32                      | 15          | 16          | 34            | 38            | 7                | 7                     | 14    | 13     | 40      | 22            | 32                      | 31                  | 30                 | 20                      |
|                                                          | 1929-30 | 30            | 32          | 34                      | 12          | 20          | 42            | 42            | 8                | 7                     | 11    | 12     | 34      | 27            | 34                      | 35                  | 25                 | 23                      |
| Fibre-length<br>(10 <sup>-2</sup> in.)                   | 1926-27 | 89            | 81          | 97                      | 80          | 78          | 82            | 97            | 68               | 66                    | 73    | 77     | 87      | 81            | 92                      | 93                  | 76                 | 83                      |
|                                                          | 1927-28 | 89            | 86          | 94                      | 77          | 79          | 89            | 94            | 73               | 70                    | 75    | 74     | 86      | 81            | 93                      | 88                  | 87                 | 85                      |
|                                                          | 1928-29 | 87            | 82          | 96                      | 80          | 76          | 88            | 96            | 69               | 71                    | 78    | 76     | 89      | 83            | 93                      | 91                  | 90                 | 81                      |
|                                                          | 1929-30 | 87            | 84          | 97                      | 75          | 81          | 92            | 100           | 69               | 68                    | 70    | 75     | 87      | 81            | 89                      | 91                  | 85                 | 83                      |
| Hair-weight per<br>centimetre<br>(10 <sup>-2</sup> mgm.) | 1926-27 | 228           | 200         | 223                     | 294         | 150         | 116           | 109           | 304              | 337                   | 268   | 270    | 204     | 223           | 173                     | 213                 | 196                | 215                     |
|                                                          | 1927-28 | 226           | 187         | 232                     | 211         | 207         | 149           | 158           | 374              | 340                   | 273   | 252    | 196     | 215           | 183                     | 218                 | 229                | 205                     |
|                                                          | 1928-29 | 204           | 182         | 228                     | 267         | 250         | 139           | 136           | 338              | 363                   | 251   | 263    | 186     | 207           | 190                     | 208                 | 209                | 208                     |
|                                                          | 1929-30 | 234           | 186         | 208                     | 232         | 201         | 176           | 164           | 333              | 356                   | 256   | 265    | 194     | 191           | 195                     | 212                 | 232                | 219                     |

TABLE VIII.  
RESULTS OF ANALYSIS OF COVARIANCE FOR 68 INDIAN COTTONS.

| Source.          | Degrees of freedom. | Correlations.                             |                                          |                               |
|------------------|---------------------|-------------------------------------------|------------------------------------------|-------------------------------|
|                  |                     | Highest standard counts and fibre-length. | Highest standard counts and hair-weight. | Fibre-length and hair-weight. |
| Varieties... ..  | 16                  | + 0.931                                   | - 0.88                                   | - 0.80                        |
| Seasons ... ..   | 3                   | - 0.93                                    | - 0.04                                   | + 0.63                        |
| Residuals ... .. | 48                  | + 0.48                                    | - 0.07                                   | + 0.11                        |

From Table VIII it will be seen that the correlation between counts and fibre-length is very high for the different varieties; it is also very high, but actually negative, for the different seasons, but as there were only four seasons and the seasonal variances are small both in counts and length, and as, moreover, the correlation between the residuals is as high as 0.48, it would obviously be unwise to attach much importance to this result for the different seasons. The negative correlation between counts and hair-weight is apparently due almost entirely to different varieties, as there is very little correlation between these properties for the different seasons and for the residuals.

To sum up: in spite of the discrepancies between the results obtained from the analyses of the three groups of cottons, they all demonstrate to a greater or less extent the inadequacy of the commonly measured fibre characters for the accurate prediction of spinning value. Some improvement may come from a better use of the measurements of immaturity, as suggested by Peirce and Lord, but in any case it appears fairly certain that before the problem of the relation between spinning value and fibre characters can be completely solved the effects of variety and environment must be disentangled. The comparison of the results for the Empire cottons with those for the Indian and Egyptian cottons suggests that environment and variety may affect the spinning value of a cotton in different ways; and it seems not improbable that spinning value is influenced by some factor or factors not yet taken into account and of great importance for the Empire cottons. If this unknown factor or factors can be correctly associated with environmental, as distinct from variety, effects, progress would undoubtedly be accelerated by an exhaustive study of a number of varieties each grown under different conditions. Such material would be extremely valuable in a number of other directions, such as in the study of immaturity and nep. Some examples of these effects have indeed already been mentioned, but much more extensive data are required; yet another opportunity is thereby provided by which cotton breeders can be of valuable assistance in growing these varieties

under a wide range of different environments ; variations in the spinning value of such series of samples could then be attributed with much greater certainty to environmental influence, and a comprehensive study of other hair characters should yield some most interesting results.

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DR. A. J. TURNER said that the paper summarised the results of the efforts made during the last ten years by Mr. Underwood and himself at the Shirley Institute and the Technological Laboratory, Bombay, to solve the problem of the relation between fibre properties and spinning value by means of the method of correlation.

The work had not been as completely successful as had originally been hoped, but interesting results had been obtained, and it had been absolutely essential to traverse this particular ground. It was disappointing that even now it was not possible to predict a spinning value from the fibre properties with any considerable degree of accuracy, especially as the spinning value itself was rather a gross criterion of the value of a cotton, as two cottons having the same spinning value might have very different values for weaving purposes.

He then briefly summarised the three sections of the paper, emphasising that, as shown in Part I, it was necessary that there should be a wide range of each property in the universe of samples examined. After referring to various questions raised by the correlations in Part II, he dealt at some length with the considerations arising out of Table VI, indicating the percentage of variance which is accounted for by different properties and different combinations of properties. It was not possible to be satisfied with the degree of success attained. He pointed out that although the tests were made on 95 Indian samples, 165 Egyptian samples, and 206 Empire samples, difficulties were occasioned in sampling the raw material for the fibre tests and in testing the yarns for the assignment of spinning value, and in processing each cotton in the manner most suitable for it ; but these difficulties could not account for the anomalies in the prediction of spinning value.

The method of linear correlation had been used for simplicity, but they did not believe that the use of non-linear correlations would have led to any improvement that was worth while. Possibly some other fibre properties not measured were of importance. It was for this reason that at the Technological Laboratory he had had tests made by an improved Adderley method on the clinging power of 45 samples which showed differences in spinning value that could not be accounted for in terms of the other fibre properties already measured ; however,

although very high correlations were found between the clinging power thus determined and some of the other fibre properties, the differences of clinging power did not in any way account for the differences in spinning value. The variability of each fibre property in any one sample and the waviness or the crinkle in the fibre were also mentioned as likely to be important factors.

Dr. Turner then referred to the questions asked in the paper concerning the comparative conditions of growth for the Egyptian and Empire varieties, and finally indicated certain specially grown cottons with which they would like to be supplied in order to carry still further the attack on the problem of the relation between the fibre properties and the spinning value. These samples were :—

- (1) Five samples, of a single strain, of the same length but different hair weights.
- (2) A similar series of five samples having the same hair weights but different mean fibre lengths.
- (3) A series of samples representing short-period pickings from a crop of plants sown at the same time.
- (4) As (3), with the plants sown at a later date, the difference between the dates of sowing being approximately equal to the period of fibre-lengthening (*i.e.* three to four weeks.)

Dr. Turner thought samples (1) and (2) could probably be obtained under irrigated conditions by suitable adjustment of the waterings, and concluded by inviting the views of those present on the possibilities of supplying material of the kind indicated.

#### DISCUSSION.

DR. W. L. BALLS said that while he greatly admired the skill with which Dr. Turner had utilised the correlation methods in the study of the problem, he was also glad that he was now turning to hoe another row, because he had felt all along that the direct experimental method was bound to be more analytical than a correlation treatment. The correlation treatment was an inevitable step, but from the nature of the case a correlation treatment was an unanalysed treatment.

*He wished Dr. Turner success in the attempt to procure comparable material ; he did not believe that the difficulties in getting that material would prove in some districts to be so colossal. Certainly in irrigated countries with the combination Dr. Turner had very wisely suggested—a variation in sowing date, with short period pickings and a linkage of everything with dates of irrigation—he did not see why the material should not be obtained. Dr. Balls said that the graphs in his book, “Raw Cotton” showed that in its most elementary form it could*

exist, and they also showed how hair properties varied ; the same thing more fully analysed was described in the Technical Bulletin which he had mentioned that morning. He was inclined to think that instead of preparing five samples, the more practicable method would be for the grower to prepare 100, out of which Dr. Turner could choose the five which happened to match the conditions he wanted. It was rather a question of the resources available to the grower in settling whether the treatment he had given and the dates he had chosen for his operations had produced the effect that Dr. Turner wanted, and such examination was more easily made at the Shirley Institute than in the field laboratory.

Dr. Balls said that the discussion again emphasised the necessity for closer liaison and actual personal interchange between the Corporation and the Shirley Institute. It was very desirable that cotton growers should have opportunities to work for a "study period" in the Shirley Institute, and that the Shirley staff should have opportunities to go to Africa and elsewhere to work similarly with the growers.

DR. PICKARD said that the role assigned to the Shirley Institute in connection with spinning tests had changed during the past year or two. More material must be examined, and the closer personal contact for which Dr. Balls had appealed was extremely desirable. The Institute was always available to anyone who came to this country, and they were always glad for cotton workers to come there and discuss their problems. As regards Dr. Balls's suggestion that opportunity should be given for members of the Corporation's staff, or other workers from overseas, to spend a period of study leave at the Shirley Institute, they would gladly provide the laboratory space. In regard to his suggestion that the staff of the Shirley Institute should have opportunities to go and work with the growers in Africa or elsewhere, he could only say that he personally agreed that it was desirable.

MR. SLATER agreed with Dr. Turner that to use spinning quality only as the criterion for grading cotton, and to ignore manufacturing and finishing qualities, was not satisfactory. The correlation of yarn strength with fibre properties was in itself a very large problem which had to be investigated, but high yarn strength alone, although of very great importance in the making of sewing threads, was of lesser importance in the majority of fine spinning productions.

He felt that the present methods of measuring clinging power or inter-fibre friction were unsound, and did not see how that property was to be satisfactorily determined except under conditions which were similar to those obtaining in yarn.

He further agreed with Dr. Turner that it was a mistake to ignore the fibre-to-fibre variability in the analysis. He would in fact go further and say that the variability of inter-fibre extension and



elasticity ought to be incorporated. It was conceivable that the strongest fibre in the cross section at the breakage point of the yarn would break before neighbouring weaker fibres, if it was the least extensible.

Finally, he expressed his admiration of Dr. Turner's tenacious grappling with this difficult problem, and he was sorry that the results were a disappointing reward for such a praiseworthy effort.

DR. TURNER asked whether the cottons grown in Africa generally, including Egypt, differed very greatly from one another compared with the soil and climatic variations.

In answer to Dr. Turner's question, MR. PARNELL replied that the strains did not differ so much in themselves, when grown under good conditions as any one strain would differ when grown under good conditions and under bad conditions respectively.

MR. TROUGHT said he would do what he could to provide Dr. Turner with the type of sample he required. The difficulties, on account of the larger samples necessary, were correspondingly greater than those in obtaining samples for Dr. Peirce.

## BROKERS' REPORTS ON COTTON SAMPLES.

BY

H. R. HOSKING (*Department of Agriculture, Uganda*).

In reports received on samples of new strains sent to England for examination, it has been found that the brokers' report and the spinning test result are frequently at variance.

To quote two concrete cases: First, a duplicate set of samples, consisting of two strains A and B, grown at different places under varying conditions of soil and climate, was submitted to two brokers. The opinions of these two brokers showed agreement in only 30 per cent. of the samples. Secondly, in Uganda there is a certain strain of cotton which has repeatedly received unfavourable reports from the brokers, and yet it has just as repeatedly been the best according to the spinning tests made by the Shirley Institute. It would seem doubtful whether such a cotton would receive its true value if put on the market in large quantities. Surely this is an anomalous position?

When conflicting evidence is received, as in the above examples, the cotton breeder is forced to rely on the machine-produced results of spinning tests. The position is made more difficult, however, when one realises that it is practically impossible

## BROKERS' REPORTS ON COTTON.

to produce enough lint for such tests from all the strains one is dealing with at any one time. The breeder, therefore, has a lurking suspicion that he may be abandoning many valuable strains because he cannot obtain reliable information as to their value from such small samples.

Hair weight per centimetre tests are valuable in this respect, but the time factor is all-important. It is impossible for a single-handed cotton breeder to make a sufficient number of these tests. Here is a case where the Shirley Institute might be in a position to render valuable assistance.

Dr. Lawrence Balls has a wide knowledge of these problems. We should be pleased to hear of any progress which has been made in the mechanical methods of classing cotton since the publication of his "Studies of Quality in Cotton."

MR. HOSKING, in introducing his paper, said that the subject might be rather controversial, but it was nevertheless an important one for a lonely cotton breeder who was trying to find some method of rejecting unsuitable cottons in an early stage.

He felt that the value of brokers' reports had been over-estimated in the past; he was referring not only to reports obtained from brokers at home, but also to opinions given by classers in Uganda.

## DISCUSSION.

DR. PICKARD explained that all cottons sent to the Corporation for test were first examined by a Committee of spinners, who assessed their value and gave their views as to the highest counts for which each sample was suitable. The impression he had formed was that spinners were usually able to assess the most suitable counts for cottons with which they were familiar, but that for new cottons they almost invariably under-estimated. He thought that the same probably applied to brokers also. Further, he believed, as other speakers had said, that when a cotton was first submitted for a broker's opinion it was a picked selected sample, but that in later years the commercial bulk was inferior and did not receive such a good report, and that this naturally caused disappointment.

DR. BALLS said that Mr. Hosking's position was the same as that in which he (Dr. Balls) had found himself in pre-war days in Egypt, when the one court of appeal as to whether a cotton was good, bad or indifferent was the Alexandria grading expert. Those Alexandria graders were marvellously good within the limitations in which they had to work, but, as the Chairman had said, when dealing with a new cotton they were completely at sea.

*When working with the Fine Spinners' Association after leaving Egypt he accumulated a number of concrete cases of contrasts between the grader's opinion and the actual behaviour of the cotton in the mill, which was the only final test; some of the more striking were mentioned in his book, "Studies of Quality in Cotton," to which Mr. Hosking referred in his paper. Since returning to Egypt, he could give ten examples, equally well documented, for every one that he gave in his book.*

*The Ministry of Agriculture in Egypt were now retaining an official consultant grader, but he was an expert who had worked with the Cotton Research Board for many years. He realised what was known about the fallibility of his judgment, and was au fait with the history of all the discrepancies. He continually checked his opinions against spinning results, and was interested rather than distressed when, as often happened, those two things did not agree. On the other hand, he was invaluable in giving an impression of the feel of the market, of saying how any new cotton was likely to be regarded during the time that it was making good, and at times he had been definitely responsible for their concentrating on some particular cotton: Giza 7 was one such instance. The grader's judgments were checked by spinning tests and in other ways, and the position was wholly different from what it was in pre-war days. But, however expert the grader, Dr. Balls said he would never base any decision of policy with regard to the introduction of a new cotton on his estimate. Graders could only give an opinion; one could only proceed safely in cotton work with a fact.*

*MR. SLATER said that in his view brokers' and spinning test reports on the same cottons represented two entirely different judgments. He had seldom been able, using spinning test data, to agree with the broker's valuation, and sometimes the disagreement was very marked. The grader, in assessing the value of a cotton, considered properties which were not included in the spinning test valuation. It might be that the grader's long experience enabled him to appreciate those qualities which would attract the spinner, and the spinning tests as at present constituted did little more than judge the cotton from the viewpoints of waste and capacity to spin a strong yarn. The waste was important for all classes of spinning, but extra strength was not always an important property, especially as the fine spinner's range of counts was considerably coarser nowadays. The time was at hand when the valuation of manufacturing and finishing quality was necessary to supplement the bare spinning test. Until this had been done, it was not quite sound to compare brokers' and spinning test reports.*

*DR. BALLS said he did not think Mr. Slater's point was really valid so far as Giza was concerned, because they did not proceed on spinning test values alone. In addition to the results of such tests, their practice was to send round to possible users, when the consideration of the*

*introduction of a cotton was in its early stages, kantar samples, bale samples and ten bale samples, and so examine the market for individual preferences and special demands, in addition to the direct spinning test. To take the example of a white cotton to replace Tanguis ; almost any spinning would do if one had the true white which characterised Tanguis.*

MR. SENNITT said that when Maarad cotton was first introduced, it was four or five years before he could persuade spinners as to its value. He received more encouragement from brokers, especially from those in Alexandria, while the continental spinners gave more encouragement than did the English spinners.

### THE EFFECT OF TYPE OF GIN ON THE SPINNING PROPERTIES (AND HENCE VALUE) OF UGANDA COTTON.

BY

H. R. HOSKING (*Department of Agriculture, Uganda*).

The most widely used gin in Uganda is of the single roller double acting type. A few ginneries have installed double roller gins in recent years. Both these gins embody a " beater " action, and, for this reason, doubts have been expressed as to their suitability for Uganda cotton. (Uganda staple varies from  $1\frac{1}{8}$  ins. to full  $1\frac{3}{16}$  ins.) It is understood that the type of gin first quoted was originally designed for a much shorter staple than that produced in Uganda.

A highly efficient single roller single acting gin with roller bearings is now on the market. It is suggested that this type would prove more suitable for Uganda than the two types mentioned above.

No data are available for comparison of the spinning qualities of samples ginned on all three types. Perhaps the Shirley Institute can give us their ideas on this subject.

So far the Department of Agriculture has not carried out any tests on these types of gin. I am not aware of any trial with the new single acting gin having been made in Uganda, although at one ginnery I saw a " home-made " single acting gin at work. This had been constructed from the old type of double acting. It appeared to be ginning extremely well. The lint was more " even-running " than that produced by a neighbouring double acting gin. The pull was better, and it seemed as if there was less damage to the staple.

Many of the ginneries in Uganda are old. Others are going out of use with centralisation. It is possible that new gins will be purchased in many instances in the next year or two. In these circumstances I feel that we should be in a position to advise the ginneries as to the most suitable type of gin to instal.

For purposes of reference I append a table summarising what appear to me to be the chief advantages and disadvantages of the three types for use under Uganda conditions.

#### DOUBLE ACTING SINGLE ROLLER.

##### *Advantages :—*

- (a) Familiar to the majority of gin-fitters.
- (b) Spares cheap and easily obtained.
- (c) Owing to low roller speed, there is little vibration. This is important, as nearly all the Uganda ginneries are constructed with the gins of the first floor.
- (d) Very efficient for ginning low grade and stained cotton.

##### *Disadvantages :—*

- (a) Multiplicity of moving, and hence wearing, parts.
- (b) Beater action possibly unsuitable for Uganda staple.
- (c) Bearings apparently absorb undue power and cause loss of time when they need refitting.
- (d) Unless great care is exercised it is not easy to prevent lint from becoming oil stained.
- (e) Adjustment is not easy.
- (f) Seed liable to be broken unless care is taken.

#### DOUBLE ROLLER.

##### *Advantages :—*

- (a) The appearance of lint produced by these gins is, on the whole, superior to that produced by the Double Acting. This is probably due to the stain being dispersed by the violent beater action.
- (b) High output, although the output of one double roller gin is not found in ordinary practice to be equivalent to that of Double Acting single roller gins.
- (c) Are probably easier to adjust once the trick is learned.
- (d) The risk of oil-stained lint seems to be less.



## TYPES OF GIN USED IN UGANDA.

*Disadvantages :—*

- (a) “Rocker” action causes high vibration.
- (b) Spares costly.
- (c) Unfamiliar to the majority of gin-fitters.
- (d) Beater action possibly unsuitable for Uganda staple.
- (e) Seed liable to be broken unless care is taken.
- (f) High initial cost—probably offset by the higher output as compared with a single roller.

## SINGLE ACTING ROLLER BEARING.

*Advantages :—*

- (a) Very easily adjusted.
- (b) Very little risk of oil-stained lint.
- (c) Needs less power in proportion to output than either of the types mentioned previously.
- (d) Produces very even running lint of nice appearance and with superior pull to that produced by the double acting.
- (e) Less chance of broken staple and broken seed.
- (f) Low wear and tear.

*Disadvantages :—*

- (a) Not known to Uganda gin-fitters.
- (b) High cost.
- (c) Few or no data available as to behaviour with Uganda cotton.
- (d) High roller speed and consequent vibration.

A fourth type of gin not previously mentioned is the saw gin. These machines are useful for ginning stained and low-grade cotton, but owing to the loss of premium as compared with roller ginned lint, they need not be considered very seriously. It should be noted, however, that when the basis is very low it is possible to compete with roller ginned lint, owing to the low cost of production. One of their greatest disadvantages is that they damage the staple and cut the seed unless very carefully adjusted.

It is felt that the standard of ginning in Uganda is not always above reproach. This is due to two main causes, viz.:—(a) Use of old and inefficient machinery in many ginneries, and (b) lack of care and understanding on the part of many Indian ginnery managers and their gin fitters.

The introduction of improved and simpler gins might improve the position.

The views of the members of this Conference would be extremely welcome.

MR. HOSKING, in introducing his paper, said that in Uganda the majority of ginneries had their gins on the first floor. He believed that started originally because the early ginneries were placed near swamps. If one put double roller gins with a very violent beater action on the first storey of a building not designed for it, it rocked and was liable to fall down unless precautions were taken, and the great vibration made it almost impossible to keep the gins in proper trim, unless the expensive precaution was taken of building concrete plinths for each gin.

He asked if anyone present had any experience of differences in value brought about by good ginning and bad ginning, and if they had any experience of comparisons of the three types of gin described in the paper.

In the paper he referred to a home-made single acting gin that he had seen at work. It was adapted from an old double acting type. The difference in the way in which the lint came from the single acting, as compared with the double acting, gin was very noticeable; there was less breakage of seed and therefore fewer of the neps with which Miss Clegg's paper had dealt; another good point was that what stain there was was diffused through the cotton and did not come out in lumps—the output was also far better. He believed that a good deal of the Sudan crop was ginned on gins of this type at Port Sudan.

#### DISCUSSION.

MISS CLEGG said they had received at the Shirley Institute about three special enquiries of which she gave the results. The cotton tested was a Sao Paulo cotton from South America, ginned on a saw gin, a single acting gin and a double acting gin respectively. The length was the same, and the amount of short lint was about the same. Samples of the cotton were put through the analyser. The analyser is normally used to take out the impurities, but it was used on this occasion to make a web, and it was found that the saw ginned cotton gave a much neppier web than the other two.

Miss Clegg went on to mention that they had received an enquiry concerning gin cutting. They had themselves never seen any sample of cotton which had been gin damaged, so they asked a firm of gin manufacturers to try and damage a West African cotton on a saw gin. They then examined two samples of ginned cotton from the same cotton, one with the gin normally fed and the other over-fed. The sample of lint produced under conditions of over-feeding was stringier than the normal product, but tests showed no significant difference of staple between the two, nor between them and some lint which they had hand-ginned themselves. After that the hairs were examined microscopically, to see if there were any difference in the number of tips and bases broken off, but, again, both the satisfactorily ginned and what was supposed to be the unsatisfactorily ginned (the over-fed, stringy cotton) gave about the same number of broken tips and bases.

In some work done by Dr. Turner on the Murray gin, roller gin and saw gin, the Murray gin gave a higher out-turn, cleaner cotton and slightly longer lint, but there was a reduction of 4 per cent. in thelea strength in yarns from the Murray-ginned cotton.

MR. SENNITT said that he had both roller gins and saw gins in Portuguese East Africa, and would be pleased to send samples from both types to the Shirley Institute for comparison. At Miss Clegg's request, he also promised to send some seed cotton from each type as well.

SIR WILLIAM HIMBURY said that in Uganda greater supervision of ginneries by the Government was required. At present they were allowed to run when the rollers were worn or the knives needed resetting. If gins were carefully adjusted, a good type of cotton could be obtained from either style of gin for American type cotton, but a roller gin was necessary for Egyptian type cotton or for Sea Island. Undoubtedly a roller gin produced actually better fibre also, if used with the type of cotton grown in Uganda, or with the shortest cotton in the world, namely, Indian cotton; but spinners showed a certain preference for saw ginned cotton, probably on account of its improved appearance, owing to the fact that the saw gin removed a certain amount of impurity. The spinners were also accustomed to buying American cotton that had been saw ginned: if Empire-grown cotton of the American type were saw ginned, its appearance resembled more closely the type of cotton which spinners were accustomed to buying from America.

MAJOR CAMERON suggested that it was undesirable to use a saw gin with cotton over a certain length of staple. He believed that about  $1\frac{1}{8}$  in. to  $1\frac{1}{4}$  in. was the limit beyond which the fibre was damaged unless a roller gin were used.

DR. TURNER replied that in the experiments that he carried out the staple of the sample ginned was  $1\frac{1}{8}$  in. A number of tests had been made

comparing saw ginned and roller ginned cotton: sometimes one type showed to advantage and sometimes the other. No experiments had been made on the effect of different types of roller gin, but he would arrange to carry out tests if Mr. Hosking could send samples ginned on the three different types of roller gin that he had described in his paper.

MR. HOSKING, replying to a question by Professor Roberts, said that when he said in his paper that the output of one double roller gin was not equal to that of the double acting single roller gin, he should have made it clear that he was speaking of the output per roller.

PROFESSOR ROBERTS said he could not agree with Mr. Hosking that double roller gins were easier to adjust. The Indian mechanic always demanded higher wages for looking after that type.

MR. HOSKING said that their experience in Uganda also with Indian mechanics was the same, but he knew of European fitters who preferred the double roller gins.

PROFESSOR ROBERTS said that in India they found that double action gins were more efficient in output than the single action type, and he suggested to Mr. Hosking that a study of the economic cost of ginning with the various types might be useful. His costs for ginning, baling and pressing, including depreciation, worked out last year at between 0.25 and 0.33 pence per pound. He believed that the cost of ginning was higher in Africa than it was in India.

MR. HOSKING said in reply that the cost of ginning in Uganda had come down very considerably in recent years. He believed the Carter Commission Report put it at the rate of 26 or 28 cents per pound of lint. He knew, however, of a ginnery where the costs had been reduced to approximately half that amount. This had been made possible by centralization and reduction of motor transport. He was not satisfied, however, that the double acting single roller gin was the cheapest to run in Uganda. The amount of time wasted in fitting and adjusting bearings on a double acting gin was considerable. Most ginneries in Uganda, if they had 12 gins, allowed for only 11 to be working at a time; one gin was considered to be always out of action.

## THE TOBLER BALE SAMPLER FOR LINT GRADING PURPOSES.

BY

H. R. HOSKING (*Department of Agriculture, Uganda*).

The Uganda Department of Agriculture would welcome the opinion of this meeting, and particularly the opinion of those members of the Shirley Institute present, as to the possibility of using Dr. Tobler's Bale Sampling Machine for lint grading purposes.

## THE TOBLER BALE SAMPLER FOR GRADING.

This machine was originally designed for the determination of the moisture content of baled cotton. Incidentally it also demonstrates cross packing extremely well.

If a core of larger diameter, or even of square cross-section, were used, would it be possible to utilise this machine for lint grading and classing purposes?

The usual practice is to break open a bale selected at random. This is costly, as one needs a baling press to re-press the opened bale. The Tobler Sampler would be considerably cheaper, both in first cost and in cost of operation. Also, it would be possible to sample a very large proportion of the bales produced by, let us say, Uganda, if the machine were located at Mombasa.

At present the problem of grade is attacked from the other end. Considerable pressure is brought to bear upon the buyers when purchasing seed-cotton from the native growers. By this means it has been found possible to improve the grade of cotton.

A few cases of inefficient ginning have come to the notice of the Department of Agriculture: these ginneries were stopped until the machinery was put into proper working order.

Such methods of control are costly to Government, as a considerable amount of travelling is required if the large number of ginneries and buying centres are to be properly supervised.

By placing a lint-grader at Mombasa—the only practical port for exporting bales—with facilities to sample effectively a large number of bales, it is felt that better control could be exercised both on the grade and on the methods of ginning and baling the Uganda crop. These suggestions are purely tentative.

We would like to hear how this problem is attacked in other countries.

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MR. H. R. HOSKING, in introducing his paper, said that in recent years the grade of Uganda cotton had dropped off badly. Last year the Agricultural Department had found that many ginners and buyers were buying dirty cotton, and they had prosecuted them, with the result that in the season just finished the grade had improved beyond all recognition. Mr. Hosking explained that he could only speak of the Eastern Province, as he did not see the Buganda side.



That was a very expensive and a haphazard method of improving the grade. There were many ginneries and many buying posts, and it entailed officers travelling round examining the buying posts, and then taking the offenders to court and probably wasting time while the case was tried. The suggestion had therefore been put forward, very tentatively, that they might find some system of attacking the problem at the other end by taking samples from the bales before export. At one time it was suggested that there should be a lint grader stationed at Mombasa, who could sample quite a good proportion of the bales exported from Uganda. That suggestion was made when production was under 200,000 bales; it had since increased to over 275,000.

Mr. Hosking suggested that the Tobler bale sampler might be used for this purpose. It was a very simple apparatus: the bale is gripped, and there is a ratchet which screws in a tube and takes out a cylindrical sample of the cotton. One or two buyers in Uganda had examined the cores drawn from bales by this implement, and had said that they would be able to give their opinion as to the *staple* of the cotton from inspection of the core. They thought, however, that it would be very difficult to *grade* the cotton suitably from those cores from the point of view of maturity and appearance, because they needed a surface to look at.

#### DISCUSSION.

DR. PICKARD *said that the Tobler sampler was rather like a cheese sampler. It was pushed into the bale and produced a core about 1 inch wide. So far as sampling for lint length was concerned, the core gave about the same result as hand sampling of the bale, but if a grader were called upon to grade cotton for cleanliness from a sample taken by means of this apparatus, his usual judgment would probably be upset by its appearance, which was naturally different from that to which he was accustomed when looking at cotton taken by hand from a bale.*

*The Shirley Institute had recently devised an apparatus called the "Shirley Analyser" which had now been put on the market. By means of this apparatus it was possible from a very small sample to ascertain accurately the percentage of the amount of the non-lint content of a sample in any form of package, so that the Tobler sample plus the analyser would give accurate figures for this. The figures given by the analyser would require correlation with the judgment of the broker.*

*The Shirley Institute had not actually used the Tobler sampler for taking samples for moisture content. They had devised a simpler*

## THE TOBLER BALE SAMPLER FOR GRADING.

and more easily portable apparatus for that purpose, which was extremely useful in mapping out the re-gain of moisture in a bale from the centre to the outside. They had not as yet put through their analyser the core produced by their sampler, but that would be done in order to ascertain whether putting the core produced by their form of sampler through the analyser gave identical results with hand sampling of the bale and then putting the hand sample through the analyser. The Shirley Institute would be pleased to show to anyone who was interested both their sampler and their analyser. One disadvantage of the Tobler apparatus was that it was rather a heavy piece of mechanism to carry about.

- SIR WILLIAM HIMBURY thought that the Uganda Government would be better advised to continue their present practice of prosecuting all ginnerers and buyers who actually bought dirty cotton. It would be better in his opinion to endeavour to stop the practice at that stage. If such a charge were based on tests of bales at Mombasa, a difficulty at once arose that the bale might have changed hands three or four times since it left the ginnery. He believed that if the present practice were continued for a sufficient length of time, it would act as a deterrent and, as he had said, it secured that the man who actually bought inferior cotton and mixed it with sound cotton was the person who was prosecuted.
- MR. OATES said that his experience of the grading of other crops in Kenya led him to support Sir William Himbury's arguments. They had found great difficulty in tracing the ownership of bales of cotton after they had changed hands several times. He also raised the point that it would take some considerable time to grade a bale of cotton and analyse it in the way suggested, and if these tests were to be made at the port he was afraid that demurrage charges would mount up.
- MR. HOSKING said it was not the intention to sample every bale which went out of the country; they would have to be taken at random.
- MR. KING said that in Tanganyika they had legislation whereby for the first six weeks from the opening of the buying season, all cotton had to be graded before it could be sold.
- MR. HOSKING replied that similar legislation in Uganda had not proved very successful.
- MR. SENNITT described the success that had attended the introduction of grading in Portuguese East Africa. Formerly, the crop was ginned without having been graded at all, and the average price paid was 60 points off. He had instituted a system of compulsory grading into four grades, and in the first year the average price rose to 70 points on, and in the following year, with better grading, to 175 points on. He naturally did not claim that this improvement

*was entirely due to the introduction of grading: some of it might be the result of better seasons, but he was certain that grading alone had made a very great improvement.*

*Mr. Sennitt then asked for advice as to the best method of sampling bales. He was required to send a sample of every bale to buyers in Europe, who bought the bales on samples. About two-pound samples were required.*

DR. PICKARD said that for this purpose he did not think the Tobler sampler would prove suitable, because in the sample produced by that apparatus the buyer would not be able to see the cotton in the form to which he was accustomed.

MR. SENNITT said that at present he took the sample from the middle of the bale as it came into the baling box and before being pressed. He would prefer to have it in the same state as that in which it arrived in Europe. That could only be secured, however, by first baling the cotton, then cutting the canvas, taking the sample, and sewing it up again.

MR. SAMPSON said that he had once seen a small hand baling machine used for making samples. In this the pressure was sufficient to give the appearance of a sample taken from an ordinary bale, and it made a small bale about 9 in. by 4 in. containing four or five samples.

DR. BALLS agreed that the best way was probably to draw the sample from the press box and then make it up with a small screw press into a five-pound bale. Such treatment gave a very fair simulation of the condition of the commercially pressed cotton.

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## PROGRESS NOTE ON MEASUREMENT OF MOISTURE IN BALES BY RADIO TECHNIQUE.

BY

W. LAWRENCE BALLS (*Chief Cotton Technologist, Ministry of Agriculture, Egypt*).

An international agreement between the spinners and exporters of Egyptian cotton led to the establishment of a testing house at Alexandria in 1932 for the control of moisture content in export bales, which has also been found valuable for control of country bales arriving in Alexandria from the ginneries. Last season nearly a thousand tests were made each month, and a large increase in this number is expected next season.

A remarkable degree of accuracy has been attained by careful attention to details in the sampling and handling, but the method

still depends—as in other testing houses—on the breaking of every tenth bale for multiple sampling in five different layers, and this broken bale has then to be re-pressed. The latter operation costs the exporter ten shillings, over and above the cost of the testing procedure, and while the total cost is relatively trivial, and often returns a handsome profit, there is evidently much economy possible if tests could be made without breaking the bales; even if they had to be carted half a mile to a specialised laboratory for the purpose.

For two years past we have been investigating the practicability of using the high dielectric constant of water as an index to its quantity by treating the bale hoops as condenser plates; and measuring the capacity between them by noting the amount which has to be subtracted from a standard tuning condenser to re-establish resonance after adding the bale to the circuit. This ostensible “capacity” is complex; as in most radio-frequency measurements it includes resistance and inductance as well as capacity, and the former is of important dimensions on account of the high dielectric absorption of wet cotton, which has the same effect as a resistance in series. There is also a shunt resistance, or leak, between the hoops. By simple modifications of the testing circuit we can obtain data which enable all these four components to be separately evaluated for any hoop-pair in about three minutes, and we have been able to show that bales can be very sharply differentiated from one another, so far as their outer layer is concerned, down to some three inches in depth below the bale surface. Wet or dry, new-pressed or dried out, recently sprayed or sun-dried, can all be distinguished clearly. But all experimental modifications have failed to give a deep field, and in its absence the Capacitance method in the specialised form of inter-hoop measurement can only be qualitative, not the quantitative one which we need, and can only have a limited use as a rapid detector device. We have to return to less specialised and unconventional procedure, at a sacrifice of convenience.

In the course of this work we have collected much information about these highly defective “condensers,” whose properties are such as radio engineers usually avoid, and about stray capacities to earth. With this knowledge we propose to begin the investigation all over again, from its first starting-point. We shall make use of the high sensitivity and dependability of the apparatus evolved during the work on hoops, and try to design a condenser into which the bale can be inserted bodily. The quantities to be measured are small at inter-plate distances of  $1\frac{1}{2}$  metres, and the provision of an approximately straight-line field has difficulties, but none of these difficulties seems insuperable in view of the past two years’ experience, and the method would be quantitative. One minute would suffice for the complete

test, and the bale would be returned to store unaltered ; the obvious economies make the investigation worth continuing.

A note on the outstanding features of the apparatus may be of interest. The valve oscillator is controlled by a piezo-electric crystal to avoid frequency drift. A loosely coupled rejector circuit is tuned to resonance with this at its frequency of 2,000,000 cycles per second. The variable condenser of the resonator circuit is 0.000070 normally, or about half the total capacity. A variable non-inductive resistance can be put in series with it to measure bale resistance directly by Moullin's half-current method. The bale hoops or bale condenser can be put in parallel to be measured by subtraction. Switches are provided to remove capacity and add inductance, thus changing the L/C ratio, so that inductance effect, if any, can be detected by comparison. Tuning is done by a thermionic voltmeter, with a device on the tuning condenser which automatically gives the average of two equal distune positions, thus reading the resonance peak-value to half a micro-microfarad. The width of distune at half-value gives an estimate of total series resistance, while the height of the peak is affected by this, and also by any shunt resistance.

Most of the difficulties associated with stray capacities to earth and to surrounding objects are avoided by a difference method. Normally one would take the base-line readings with all earth-connections established, only the hoop or plate on the "live" side of the resonator circuit remaining disconnected. Actually the reverse is the correct procedure ; the top hoop of the bale, for example, is connected up to the live side, and its readings taken in relation to "earth in general" ; then the middle hoop is put to earth and fresh readings are made. The difference between these two gives the capacity and resistance and inductance, if any, between the top and middle hoops.

It remains to be seen whether these investigations can be brought to success in practical use, but the delicacy of the methods, and the speed with which determinations can be made, make them exceptionally interesting.

We are indebted to Mr. J. H. Cole of the Egyptian Survey, and latterly to our colleague, Mr. H. A. Hancock, for valuable help in mathematical treatment and in fundamental electrical theory.

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## MOISTURE DETERMINATION IN BALES.

DR. W. L. BALLS, in introducing his paper, said that it was merely a progress note. It arose out of the establishment of an international agreement between the Master Spinners and the Egyptian Exporters to trade in cotton of a certified moisture content. The cost of moisture testing was rather high, because of the fact that the sampling was done on every tenth bale, and the broken bale had then to be re-pressed. Although oven-testing had been found a very paying proposition, one resented spending a comparatively large sum of money if it could be done much more cheaply and much more rapidly, and he had partially wasted two years in trying to find some alternative technique. He used the words "partially wasted" only in the sense that, for the sake of the extreme convenience, he had tried to exploit a method which he had since found was not capable of giving fully quantitative results; he was using the experience he had acquired by going back to less convenient methods which he had originally contemplated.

The method which he had been trying to exploit was the measurement of the electrical capacity between the bale hoops themselves. The Egyptian export bale was a beautifully standardised thing; the error in the position of the hoops was a matter of millimetres only. He tried to treat those hoops as unconventional condenser plates and to measure the capacity between them. Dr. Balls illustrated the results by means of two diagrams.

The first was the elementary case of capacity between two hoops at different distances. If those hoops were in air, the curve was as shown in the diagram, and the behaviour was that of a true condenser. If the bale were well aged and the surface pretty thoroughly dried, again true condenser behaviour was shown as illustrated in the two relevant curves. The capacity was halved as the distance between the hoops was doubled.

Those two curves illustrated one of the difficulties. Actually the bale which had the lower capacity was slightly the wetter of the two; the difference was 5.3 and 6.2. Those bales had been desiccated for 15 months at Giza. The essential fact was that by measuring the capacity between the hoops one did not get a really deep field. The bales were in substantially the same condition on the surface, but the one showing the higher capacity was slightly wetter superficially because it was sprayed two months previously.

Taking that same bale and saturating it with water on the outside (3 lbs. of water were sprayed and water was dripping off the bale

at the time the measurements were taken), the capacity between hoops which were a long way apart was practically unaltered. With hoops which were closer together, the apparent capacity, which now included a good deal of resistance, deviated still further. If the bale were left to dry for a day it returned to the position shown on the diagram. It was by no means dry; it had been drying under cover, so that the water had gone deeper. The further the hoops chosen as electrodes were separated, the deeper the field.

Another diagram showed the case of a bale of similar weight fresh from the press, with approximately the normal  $8\frac{1}{2}$  per cent. moisture content. The way in which all the capacities had shifted up was shown, and the halving relation was imperfect. One of the mistakes he had made at the start was to assume that a freshly pressed bale was ideally homogeneous. Actually this was not so at all; the centre of the bale was partially dried by the pressure in compressing, and the outside had a damp layer which introduced more series resistance at the smaller hoop-intervals, lessening the apparent capacity.

Another diagram showed the results obtained on a few more bales with the same method of determination. Red and black heavy lines represented two bales of equal weight but differing moisture content, 7.9 and 9.3 per cent. The total quantity of water was the factor that controlled the capacity readings. Evidently in broad outline the method was quantitative, but it was not good enough in its present form.

The interest of the possibilities was that in Alexandria for about three months of the season some 17 steam presses run all day, and sometimes into the night as well, turning out bales at the rate of a bale every 63 seconds. To keep pace with that when moisture-testing became even more generally adopted would require either an elaborate organisation, or a simpler technique.

## OIL PROBLEMS.

*Chairman : Sir JOHN RUSSELL, F.R.S.*

The two following papers were introduced by their respective authors and were then discussed together.

### PHYSICAL MEASUREMENTS OF SOIL IN RELATION TO SOIL TYPE AND FERTILITY.

BY

B. A. KEEN (*Rothamsted Experimental Station*).

This paper is an interim summary of a number of separate investigations at Rothamsted which have one underlying principle in common—to ascertain what types of physical measurements on soil are most suited to distinguish differences in the natural or inherent fertility of different soils and, as a related investigation, to what extent soil types can also be distinguished by physical measurements.

The investigation, by its very nature, could not be expected to give quantitative conclusions. In the first place, the natural or inherent fertility of soils in those areas of the world that are still in substantially their virgin condition, *i.e.* not already modified by manuring, is capable of only qualitative assessment; in the second place, while the soil surveyor can usually fix with certainty the type to which a given soil belongs, a single type may often include soils whose other characteristics must almost certainly give wide differences in the results of any physical measurement, *e.g.* the same soil type may well include a sandy and a clayey soil, which have of course markedly different physical behaviour; in the third place, the physical measurements must not require an elaborate technique or apparatus, since if they are to be of practical value they must be capable of use on a large number of samples and within the meagre resources of a field laboratory.

In spite of these restrictions, the investigation is well worth while. The government or estate official who has the task of

bringing unknown virgin soil into cultivation needs every possible help in deciding which parts of the area to take in.

Evidently one of the first steps in the investigation was to ascertain whether from among the wide range of possible physical measurements it was possible to select those that gave the maximum amount of independent and relevant information. For example, if it is found that the results of two separable tests, which measure quite different properties, or groups of properties, are highly correlated over a representative range of samples, there is no point in performing both the tests: one is sufficient. A preliminary investigation was made by Keen and Coutts<sup>(1)</sup> on 39 soils of widely different origins, and the work was afterwards extended by Coutts<sup>(2)</sup> to some 66 soils, all from a restricted area in Natal, but representing several geological formations. The data from the latter set of observations was subjected by Russell<sup>(3)</sup> to an intensive statistical analysis. The original paper must be consulted for details of the procedure; it is only possible to give the main conclusions here. Several soil properties such as the sticky point,<sup>(4)</sup> the weight of water held per gram of soil in the Keen-Raczkowski "box" measurements,<sup>(5)</sup> and the moisture content at 50 per cent. relative humidity<sup>(6)</sup> are closely correlated with the base-exchange-capacity,\* while the clay content of the soil is of minor importance in predicting these properties. The prediction is only slightly improved if the silt is also taken into account. These results led to the provisional conclusion that if the quantity of colloidal material in soil is proportional to the clay content, then such physical quantities as those just mentioned are primarily controlled by the quantity of exchangeable ions and much less by the type of colloidal material present. Another result was that the xylene equivalent† measures a property of the soil that is independent of the organic matter present. This was shown by the high degree to which the xylene equivalent could be predicted from the results of other physical tests made on the soil after it had been treated with hydrogen peroxide to remove organic matter. Hence it would be expected that the xylene equivalent should be related in some way with the proportion and size distribution of the mineral portion of the soil; it was found, in fact, that it was

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\* As measured by Schofield's potassium phosphate buffer method.  
*J. Agric. Sci.* (1933) **23**, 255.

† The well-known moisture equivalent of Briggs and McLane, but done with xylene, not water, as the liquid.

more completely determined than any other variable, by the amount of clay and silt in the soil, although the actual prediction was not of a high order of accuracy.

The net result of this preliminary investigation suggested that a few relatively simple physical tests would probably suffice to grade or divide soils, and that the divisions thus effected would not be substantially changed by the additional information obtained from more elaborate laboratory tests. The position was sufficiently encouraging to justify an examination of a wider range of soils to see how far a few selected physical tests would succeed in differentiating between soils of different types and of different inherent fertility. Sets of soils were obtained, together with such information as was available as to soil type and inherent fertility, from Grenada and Trinidad, Nyasaland, the Spanish Levant, and Malaya. The Malayan samples were the most extensive and were obtained during an extensive soil survey of rubber areas.

The results obtained are discussed immediately below. A number of general relations were again found. The following were typical:—the sticky point was closely related to the weight of water held per gram of soil in the “ box ” measurement; the moisture content at 50 per cent. relative humidity was closely related to the base-exchange-capacity, with the interesting and suggestive exception that certain Nyasaland soils of a distinctly humic character fell apart from the others, having higher base-exchange-capacities than their relative humidity figures would suggest. In addition, another general relationship, derived from a separate study at Rothamsted, may be mentioned: the heat-of-wetting of dry soil is closely related to its base-exchange-capacity.<sup>(7)</sup>

But when the examination is directed to ascertain how far such measurements serve to distinguish one soil type from another, the relationships become more diffuse. Ideally, one would desire to obtain a very good correlation between two different measurements, when done on soils from the same type, and to find that the regression constants of the curve connecting the two measurements varied with the actual soil type. So far as our work has gone it appears that if the relation between two such measurements  $x$  and  $y$  depends on soil type, then it seems impossible to get close correlations between  $x$  and  $y$ . This is possibly due to the fact that within a broad soil type the sub-types are sufficiently variable to affect the correlation. In other words,



if  $x$  and  $y$  are plotted together, a soil type will possibly be characterised by an area of the paper and not by a curve. Fig. 1 illustrates this: the association of a given group of soils with a particular area of the figure is by no means exact, but there is clearly a general segregation into separate areas. Further work will be needed along similar lines and on additional soil types before the possibility of final generalisations can be decided. One possibility is that the position of the areas in a figure, such as Fig. 1, is governed by the degree of saturation of the soil, *i.e.*, by

$$\text{the ratio} \quad \frac{\text{bases contained by the soil}}{\text{bases contained by the soil at pH 7.}}$$

The assessment of inherent fertility by physical or physico-chemical tests is, naturally, a still more difficult problem. An experimentally determined numerical value is obtained from the physical test, but the value for the fertility can hardly be expected to provide much more than a qualitative estimate of good, medium, or poor. In the examinations so far conducted, the Malayan soil samples have been used, since they come from a restricted area, and the worker who took the samples was able to consult the rubber yields in forming his estimate of inherent fertility. A number of physical measurements were made on each sample, but most of those that appeared promising in the first batch of soils broke down when applied to the full series of 49 samples. One simple test, however, succeeded in differentiating with very few exceptions between good fertility soils and the remainder. This was the loss on ignition of the oven-dry soil; if the value exceeds 6.5 per cent. the soil is of good fertility. Of the twenty good soils only three fell outside this criterion, and from indirect evidence it is likely that only one of the three is a genuine exception to the criterion. The criterion also classifies as good, five of the thirteen medium soils, and three of the sixteen poor soils. If to this criterion is added the additional condition that the loss on ignition per gram of clay shall exceed 13.5 per cent., then the number of medium and poor soils still classified as good is reduced to three and two respectively. The results are shown in Fig. 2. This improvement is not sufficient to outweigh the disadvantage of the long and tedious determination of ignition loss on the clay, especially as the original criterion—ignition loss of the whole soil—is simple and easy to determine. It is not possible to say at this stage whether the result can be applied to other soils and to crops other than rubber. It is unlikely that any single test would have universal application. But if a simple criterion analogous to the

one discussed is found to apply to a locality where a crop such as sugar-cane, or cotton, is already grown extensively, it could be used with some confidence when it was decided to extend the area under the crop.

*References.*

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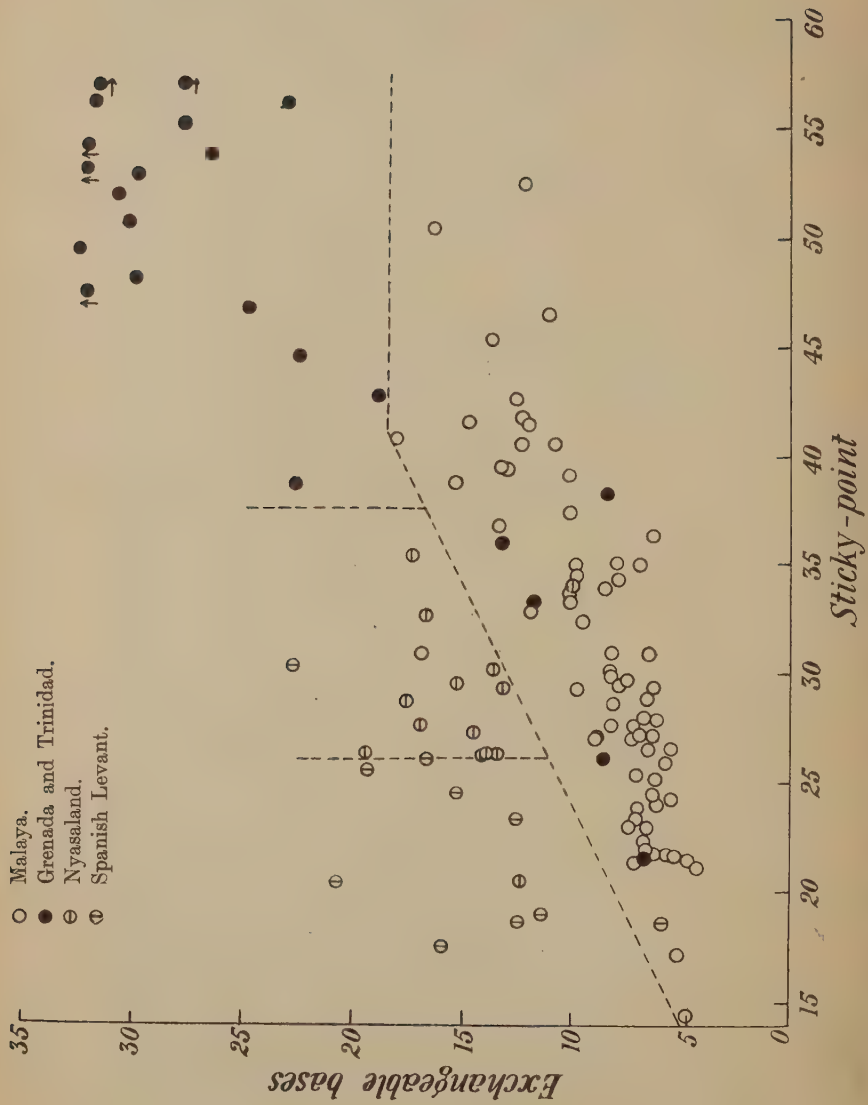


FIGURE 1.—RELATION BETWEEN STICKY-POINT AND EXCHANGEABLE BASES FOR DIFFERENT SOIL TYPES.

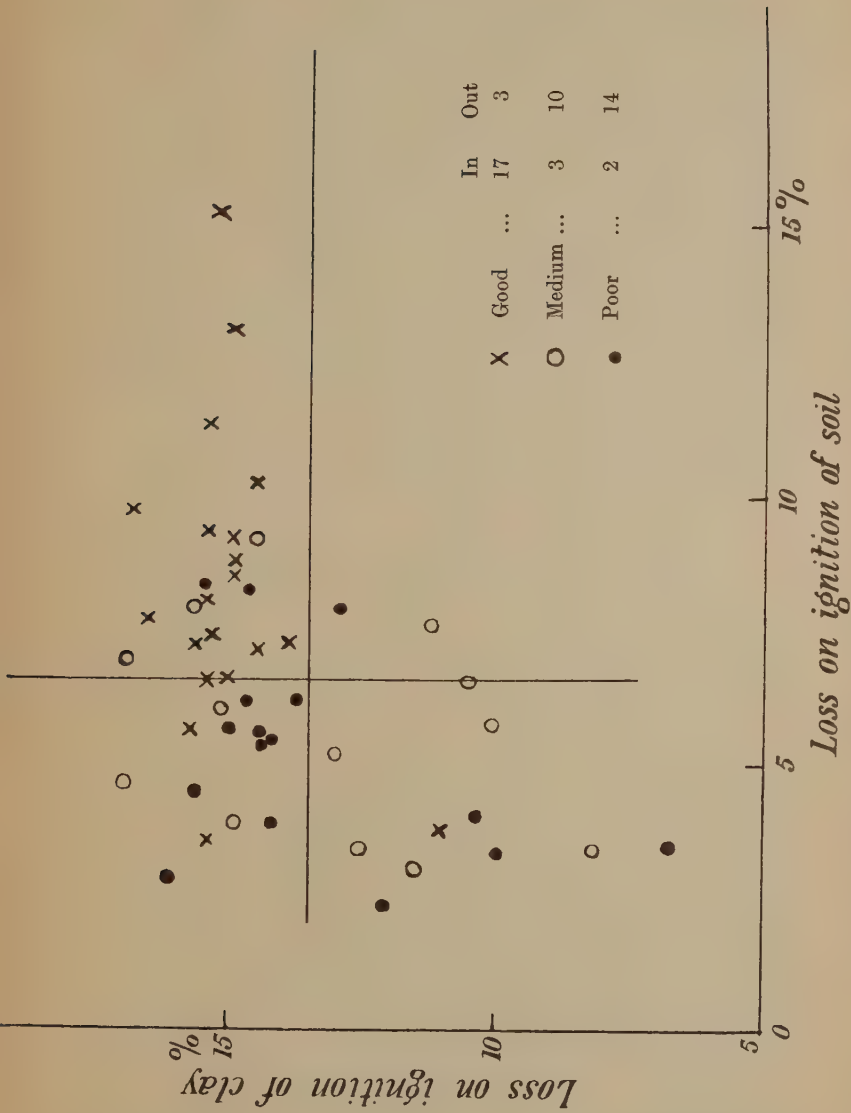


FIGURE 2.—RELATION BETWEEN FERTILITY AND IGNITION LOSS OF SOIL AND CLAY.

DR. B. A. KEEN, in introducing his paper, said that, like the preceding paper, it was essentially a progress report. Soil had a variety of physical properties, some of which were self-evident. For example, the general moisture content of the soil was closely related to its mechanical analysis and in particular to its organic matter content and the content of clay. But many measurements seemed to be the indirect expression of some physical factor or group of factors, and the question arose whether some of those measurements were expressing the same group of physical factors or whether they were measuring distinct or non-related groups. The method of investigating that problem was the well-known one of repeating the measurements on as many different soils as possible and looking for correlations, partial correlations and regressions and so on. Although such work dealt mainly with the scientific study of soil, it was desired to know if the measurements, when their import was realised, were of diagnostic use in the field for general soil surveying and classification, for the extension of planting areas into hitherto unknown soil, and so on. An example dealing with soil classification was given in Fig. 1 of the paper, which showed the relationship between the sticky point and the content of exchangeable bases.

Those two measurements were done on a variety of soils, indicated in the legend at the top of the diagram, and a glance at the plotted points showed that there was a tendency to a kind of segregation. The Malaya soils tended to fall to the right of the sloping line, the Grenada and Trinidad soils were more or less concentrated at the top, and so on. That illustrated an attempt to classify soil types by relatively simple physical and physico-chemical measurements.

Passing on to the much broader and perhaps more immediately interesting problem of how far such measurements could be used to give any indication of the natural or the inherent fertility of the soil, Dr. Keen said there was much less information. So far it had been possible to work in detail only on a rather nicely graded series of rubber-growing soils from Malaya, whose approximate fertility, as indicated by their yield of rubber, was known. On those soils a variety of the physical measurements indicated in the paper were tried with some fairly encouraging results. There was a fairly good relationship between the fertility of the soil, classed on the spot in three grades—good, medium and poor—and a very simple physical measurement, the actual loss on ignition of the soil. If that value exceeded about 6 per cent., most of the good fertility soils fell above that critical value. Fig. 2 in the paper showed that the majority of the crosses fell to the right of the upright line drawn opposite the 6 per cent. value.



The next step was to look for an additional measurement which would sort out some of the doubtful cases. For that purpose the loss on ignition of the clay fraction was of some service. If its value exceeded approximately 13 per cent., then, as shown by the horizontal line on the diagram (Fig. 2), the actual number of good soils caught in the upper right-hand quadrant was 17, and only three good soils fell outside it. The medium soils and the poor soils were also graded to a slightly better degree with the combination of these two measurements than by the use of the single measurement first mentioned.

The point of the paper was not to suggest that the methods mentioned were suitable for general application, but to encourage investigation into the possibility of using analagous measurements as a guide in cases where the area under a given crop had to be extended into, for example, an adjacent virgin area.

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## SOIL ORGANIC MATTER AND CROP ROTATION.

BY

E. M. CROWTHER (*Rothamsted Experimental Station*).

The Reports received from the Experiment Stations and several recent articles in the Empire Cotton Growing Review show that most of the newer cotton areas have reached the stage of agricultural development at which it is desirable to enquire more deeply into the effects of different crop rotations on the maintenance of soil fertility. The local conditions vary so widely that in attempting to introduce a general discussion on this subject one is inclined at first to agree with a recent Editorial comment that "each place will evidently have to work out its own rotation and it is unlikely that the work will lend itself to much in the way of generalisation." But at a Conference such as this it seems profitable to enquire whether questions of crop rotation must always remain part of the empirical art of farming. Is it possible to incorporate in the work of the cotton stations some experiments which may be used to advance agricultural science by showing how the different crops and cultivations of a rotation affect the fertility of the soil?

The obvious importance of local factors—weeds, pests, diseases, labour costs and crop values—always tends to mask the more fundamental question of crop nutrition. Problems of soil fertility and crop rotations are inevitably complex, but it is

suggested that in the tropics, and especially for cotton, they are not merely more directly important than in Western Europe, but also more amenable to experimental study.

The cotton crop provides abundant opportunity for loss of nutrient, for it needs cultivation at temperatures and moisture conditions under which soil organic matter is rapidly oxidised away. It provides no fodder or litter for stock and thus leads to unbalanced farming and soil exhaustion. In some of the older cotton areas the inevitable drain on the soil is met by heavy manuring by fertilisers, as in the Eastern cotton States of U.S.A., or through the residues of large cultivated crops, such as Egyptian berseem. Neither of these methods is practicable at present in most of the newer areas and some substitute must be found if stable systems of husbandry are to be attained. The soils start with very little organic matter, the oxidation processes in the soil are extremely rapid and the alternative rotation crops are so few that the agricultural problems can be clearly defined. The problems are less complex than in this country, where rotations, established by the accumulated tradition of centuries, have survived without much change and also without serious study. The chain of cotton experiment stations has a unique opportunity for making a really important contribution to agricultural science. They have a vital problem concerning a common annual crop grown in simple rotations under the most diverse conditions. They have facilities for accurate field trials and opportunities for learning each other's results through the activities of the Corporation and the Imperial Bureaux.

In many centres the most urgent problem is to devise means of returning organic matter and plant nutrients through crop residues, green manures, rotted manures or composts. The first step is to discover reasonably efficient rotation crops, processes of cultivation and methods of making manure. Great progress has already been made in improving methods of rotting down resistant plant material (*e.g.* the Adco and Indore processes) and in adapting these or more traditional methods to the conditions of primitive agriculture. Methods of handling green manures have been well studied in Nigeria and elsewhere. In the Sudan Gezira rotations with long fallows are favoured. In the Queensland dairying areas, as in those of this country, the obvious method appears to be alternating arable cultivations with long grass leys. Where promising rotations are already known, the next step should be to set up additional experiments which will not only compare alternative rotations, but which will give, in addition,

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some information about the way in which the crops and manures and fallows affect the soil. It is not sufficient to be content with general statements about "keeping up the humus supply," "improving water-holding capacity" or "stimulating the micro-organisms." Nor is it wise to be content with apparently simple field experiments and expect the chemist or microbiologist to do the rest by elaborate laboratory work on soil samples.

It is proposed here to discuss some of the effects of crops and cultivations on soil fertility with the object of demonstrating the need for field experiments and of encouraging discussion on suitable methods.

## ASH CONSTITUENTS OF PLANTS.

Cotton is grown successfully on such a variety of soils that it is unlikely that shortage of phosphate or potash will often prove to be a limiting factor, except in those areas, *e.g.* South Africa, where almost all crops need phosphatic fertilisers. Attention may, however, be directed to one possible effect of rotation crops, especially leguminous ones, which is sometimes overlooked. In poor, light soils in countries of high rainfall, calcium and other bases are particularly liable to loss by leaching. It happens that in many soils of East and West Africa the surface soil is less acid, *i.e.* richer in bases, than the deep subsoil. Presumably the vegetation extracts basic material from the rapidly weathering rock and restores it to the surface during the decay of roots, fallen leaves and branches. Deeply rooting cultivated crops, especially leguminous ones, may be expected to have similar effects, for they are often rich in lime and phosphoric acid. Further, some leguminous crops (*e.g.* lupins) can utilise insoluble compounds, such as apatites of mineral phosphates, which are almost useless to cereals and many other plants.

One soil scientist—Professor Williams of Moscow—has even gone so far as to consider the most important contribution of the leguminous plants of leys and grassland to the maintenance of fertility to be the restoration of calcium to the surface horizon of the soil. This may be an exaggeration, but it would be unwise to neglect the mineral elements in considering the effects of crop residues.

## SOIL ORGANIC MATTER.

All soils contain organic matter in all stages of decomposition from living plant and microbial tissues to amorphous colloidal humic materials. Unfortunately there are no satisfactory methods for fractionating or analysing this complex mixture of materials.

Even the determination of the total amount of organic matter presents difficulties, for its carbon and nitrogen contents are not constant. In general, the ratio of carbon to nitrogen in the soil organic matter tends to fluctuate around 10:1. Some soils contain notable amounts of such inert materials as charcoal and coal. The total amounts of carbon and nitrogen tend to increase in grassland and forest soils, even in the absence of leguminous plants. Under cultivation they fall at rates which depend on moisture, temperature and aeration. One-third of the total carbon and nitrogen of the soil was lost in fifty years of continuous cropping with wheat or barley on the light sandy loam of the Woburn Experiment Station. In the tropics the losses are naturally much more rapid. Except where the total amounts of organic matter are small and rates of change extremely rapid, it is almost impossible to follow by chemical analyses the annual changes in the total carbon or nitrogen of field soils. It must, however, be remembered that such processes as nitrogen fixation, denitrification and losses of nitrogen by leaching or as gas cannot be demonstrated in field soils without these difficult analyses. The presence of micro-organisms capable of affecting any of these changes is no evidence that they are in fact playing any considerable part in the production or loss of potential plant food; most of them may be found in any reasonably fertile soil.

Up to the present there is little evidence that the benefits from fermented manures and composts are to be ascribed to the actual addition of micro-organisms to the soil. Some of the recent Nigerian experiments suggest that, under conditions of intense desiccation and high temperatures, sterilisation of the surface soil may proceed so far that re-inoculation may be needed. Recent biochemical work opens up the possibility that the growth-promoting substances—auxins, hormones, etc.—may be present in animal manures. Again, some of the minor elements, which occasionally prove limiting for crop growth, are supplied, or made more available, by farmyard manure and other crop residues. There can, however, be no doubt that the main advantage from the use of organic manures depends on the slow but steady liberation of ammonia, nitrate and other plant nutrients and, where large amounts are used, in the improvement in soil tilth and structure.

It is now well known that green leaves, roots and other materials which are relatively rich in protein, decompose in the soil extremely rapidly with the liberation of much ammonia or nitrate. Such materials supply available nitrogen almost as rapidly

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as the usual nitrogenous fertilisers and should be treated with similar caution in soils liable to extensive leaching. We have argued that the failure of vetches as a preparation for winter wheat at Woburn is to be explained in part by rapid nitrification and loss during the lengthy periods when the soil is bare or carrying only a small crop. It may be that the apparent unimportance of the organic matter in the tops of leguminous plants in some of the Nigerian experiments results from similarly rapid losses when the tops are buried in the soil.

Cellulosic materials decompose very rapidly provided that sufficient nitrogen is forthcoming from other sources to meet the needs of the micro-organisms concerned. Straw and woody materials decompose more slowly and continue for considerable periods to use up the available nitrogen of the soil.

The effects of crop residues on the production of available nitrogen in the soil may generally be interpreted in terms of the composition of the materials added, the weather conditions, and the aptitude of the soil to store water and soluble nutrients. The most vital factor of all is the timing of the operations and this can be studied satisfactorily only in specific field experiments. It must never be forgotten that, in soil capable of rapid oxidation and liable to leaching, all cultivations in preparation for a new crop or in burying the residues of an old one greatly accelerate the decomposition and the risk of loss. Poor green manure crops can rarely be useful, for the inevitable losses may easily exceed any benefit from the material added.

## AVAILABLE NITROGEN.

Although nitrates occupy a unique position in the nitrogen economy of the soil, it would appear that undue attention is often given to the nitrate content of the surface soil. There are many reasons why the amount of surface nitrate cannot be regarded as a good measure of available nitrogen. Nitrate owes its unique position to the fact that it is the only stable compound of nitrogen which is not absorbed by the colloids. It is free to move up and down with the soil water and it is readily leached out. When rapid evaporation follows heavy rainfall or irrigation, the capillary rise of water from the saturated subsoil carries nitrate to the rapidly drying surface where it may remain out of the root range of the crop. Thus, the ridges of the Sudan Gezira cotton soils often have high nitrate contents when the crop is obviously suffering from nitrogen shortage. In Queensland it has been found



that in six to eight years cotton soils lose much organic matter and become markedly less permeable to water; the nitrate contents of the surface soil are actually higher than in the more fertile new soils. Here also, it appears possible that the surface soil remains sufficiently wet after heavy rains to allow capillary rise of soil water and surface concentration of nitrate.

In open soils nitrates are obviously liable to be leached away beyond the range of plant roots and it is generally recognized that fallowing or frequent cultivations may be very wasteful. In heavier soils with good soil structure, *i.e.*, with abundance of drainage channels and cracks, the gentle seepage of soil water allows nitrate and other soluble materials to diffuse into the lumps of subsoil. Heavy rains drain away chiefly through the main channels and extract the accumulated salts only slowly. It is suggested that good subsoil structure in loams and heavy soils provides the possibility for storing large amounts of water and nitrate, even under conditions of moderate drainage. Such storage and slow extraction of accumulated nitrate probably explains why in the continuous wheat on heavy soil of Broadbalk field at Rothamsted, it has been possible to relate yields to the winter rainfall, whereas in corresponding experiments on light soil at Woburn the yield and rainfall correlation are barely significant. Subsoil storage of nutrient in soils of good structure appears to be an important factor in the beneficial effect of fallowing on heavy soils.

Even without leaching or surface concentration the amount of nitrate in a soil is merely a balance between production and removal by plants and micro-organisms. Soils with large reserves of plant residues may contain little more nitrate than less rich soils, but they will continue to produce ammonia or nitrate for much longer periods. The state of "good condition" in highly farmed soils may depend in part on the reserves of readily oxidisable material. There is no method of determining these substances by direct chemical analyses, but some promising results have recently been obtained at Rothamsted by measuring the end products—carbon dioxide, ammonia and nitrate—in soil samples incubated in the laboratory under conditions favouring rapid oxidation. Soil analyses by Dr. E. R. Orchard on a field experiment with different leys and fallows in preparation for wheat showed the rapid loss of oxidisable carbon and the accumulation of field nitrates on fallowing, and also the increase in nitrifiable nitrogen by clover. The wheat yields were correlated with the nitrate contents of the

soil during the previous year, even though from mid-winter onwards the nitrates on all plots were low and equal. The wheat yields were also correlated positively with the total nitrate and nitrifiable nitrogen and negatively with the oxidisable carbon during the previous year.

#### SOIL WATER AND SOIL STRUCTURE.

Under almost all conditions of soil and climate it has been observed that soils with large amounts of plant roots, organic manure, or humus absorb and retain water better than those which have lost much of their organic matter by frequent cultivation. The opposite extreme of impermeable and eroding soils is only too well known in many parts of the tropics where clean weeding has been practised. From experience and current teachings in countries with temperate climates, some tropical workers are inclined to assign the beneficial effects of cover crops, root residues and manures to the humus they provide. But most cultivated tropical soils contain so little humus and need such frequent additions of fresh organic matter to maintain this modest amount, that it seems more profitable to neglect the hypothetical effects of the humic material and to focus attention on the growing plants, the added materials, and their immediate decomposition products, when considering the physical effects as well as the more purely chemical ones.

It seems important to ascertain whether added organic manures are as effective as the roots of growing plants in draining and aerating the soil and opening up the subsoil. At Barberton, Heath has suggested that the advantage of maize and beans over fallow in preparation for cotton may be due to better absorption and penetration of water through the channels opened up by the plant roots. He has attempted to analyse the effect by combining cultivation treatments with his rotation experiments.

#### SOIL PROFILES AND SOIL CHARACTERISATION.

In attempting to apply the results of experiments to other soils, either in the same district or more generally, it is essential to obtain some specification of the soil conditions. Too often it is assumed that this must require elaborate physical and chemical analyses. It may encourage agronomists and other cotton workers, who are unable to obtain the collaboration of a soil chemist, to know that the experience of soil surveyors and pedologists in all parts of the world has shown that the first and essential step in soil characterisation is to secure accurate description of the visible characters of soil profile down to and somewhat beyond the root range. Many cotton workers have occasion to explore the roots

of plants or have the facilities of cheap labour for digging special pits. They should never miss suitable opportunities for describing, in great detail, the colour, texture, structure and thickness or depth of the successive horizons of the soil profile, as seen in a section. They need not worry about trying to fit their observations into any of the current schemes of soil classification, for all of these are purely tentative and deal with soils of temperate climates, where little cotton is grown. The descriptive soil data obtained in the cotton areas will be useful in extending methods of classifying tropical soils, but they will be even more immediately useful in interpreting the results of rotation experiments in terms of the nature of the soil and subsoil, the depth of root range, and the water penetration and retention.

#### PROPOSED MULTIPLE FACTOR ROTATION EXPERIMENTS.

Many of the cotton stations have already appreciated the value of multiple factor experiments in which several different sets of alternative treatments are tested together in all possible combinations. The experiments are efficient, for they explore a wide range of possible treatments, and provide a large number of degrees of freedom for the estimate of error. By judicious combination of treatments much can be learned about the way in which the effects are produced.

Rotation experiments are obviously more difficult to conduct than variety or manuring trials. There can be no universal scheme, but in several cotton stations the problem can be reduced to testing fallow, a leguminous crop and a cereal as preparation for cotton in the following year. To reduce difficulties arising from isolated plots of cotton it will generally be sufficient to run the experiment in two large blocks with all the cotton alternately in one and the other. Experiments elaborated from this basis are already in progress in the Sudan and at Barberton. Since the fundamental problems are to decide how far any differences ultimately obtained are due to nutrients, especially nitrogen, and to physical effects, especially on the distribution of water and soluble salts, it is advisable to test manuring and cultivation effects after each of the preparative treatments. It is suggested that suitable manuring tests would be (1) unmanured, (2) sulphate of ammonia, and (3) some form of rotted organic manure, *e.g.* farmyard manure, Adco, or Indore compost. In the third treatment bulky material should definitely be incorporated with the soil. It is scarcely sufficient to allow a few animals to be fed on the plots months before cotton is planted, as in some of the Sudan "farmyard

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manure " experiments. The object should be to provide material comparable with the previous crops in the organic matter added and with the sulphate of ammonia in the amount of nitrogen supplied. Since the fallow and the crops may have different effects on tilth and structure, it is advisable to add two cultivation treatments, such as ploughings at as widely separated times as possible. The scheme would thus be all combinations of

2 years  $\times$  3 crops  $\times$  3 manurings  $\times$  2 cultivations = 36 treatments.

Three or fourfold replication in 18-plot blocks would be necessary. Although the whole of the cotton plots could be kept together, it is advisable to put the other treatments at random within the blocks. Split plots should be avoided, wherever possible, for they generally lead to lower accuracy for the major questions than for the minor ones. In addition, they cause trouble in the analysis, statement and discussion of results.

Such experiments might be continued for several rotations to bring out seasonal and cumulative effects, but repetition in this way would not reduce the effects of local positional and soil irregularities. Sometimes it will be desirable to study in more detail and with greater accuracy the immediate effect of one treatment on the following crop without introducing those cumulative effects which sometimes amount to serious disturbances of soil fertility. Thus, if in a six year rotation : cotton, leguminous plants, cotton, cereal, cotton, fallow, each stage were tested each year and each plot passed through the full cycle (though not in the same order), by the end of six years every plot would have been used to give information on the effects of each crop. The effects of soil irregularities and position would be minimised and the fertility of the whole area would remain approximately uniform. In a long-period cultivation—manuring experiment recently laid down at Rothamsted, half of the plots are intended to bring out the cumulative effects by repeating the treatments on the same plots, and the other half to give the immediate effects without accumulation by rotating the treatments through a cycle.

It is not suggested that these schemes should be adopted as they stand—the Sudan workers might not like the farmyard manure nor the Nigerian ones the fallows. Local adaptations based on similar principles should serve to give the whole subject of soil fertility and crop rotation a firm foundation of definitely ascertained facts in place of the shifting sands of hypotheses derived from empirical agriculture or laboratory science.

DR. CROWTHER, in introducing his paper, said that his main purpose was to raise the question whether it was possible, in the newer cotton areas, to plan experiments which would not merely compare alternative rotations, but, in addition, would show how the individual crops affected the fertility of the soil as shown by the yield of cotton.

Such information was needed urgently in the cotton areas, and the problems involved could be attacked more directly and more hopefully than the corresponding ones in this country. The cotton crop provided little opportunity for returning organic matter to the soil; it led to unbalanced farming. The rotation crops available were generally few, and relatively simple experiments could be planned to provide both practical and scientific information.

The paper reviewed some of the effects of crop residues and organic matter on the soil. The possibility of returning important ash constituents to the surface soil through crop residues should not be neglected, especially in highly leached soils such as those characteristic African soils in which the surface soil was less acid than the deeper subsoil.

It was essential to have adequate field experiments and not to be content with simple field trials in the expectation that the microbiologist or soil chemist could do the rest. Proof of nitrogen fixation, denitrification and other gains or losses of soil nitrogen depended entirely on the results of difficult soil sampling and analysis. In particular, too much stress should not be laid on the carbon-nitrogen ratios of soil organic matter, unless it was known that the analytical methods employed were in fact reliable. The probable rate of decomposition of crop residues could be estimated sufficiently well from their carbon-nitrogen ratios, or, more simply, from the nitrogen content of the dry matter. It should be remembered that plant residues, which were rich in protein, decomposed almost as rapidly as sulphate of ammonia. The most vital factor of all experiments on green manures and crop residues was the actual timing of the cultivation operations in relation to the weather and the following crop. These problems could be studied satisfactorily only in specific field experiments. The losses inevitably caused by cultivation might easily mask the beneficial effects from the plant nutrients returned to the soil.

Nitrates in the surface soil probably received undue attention through the relative ease of sampling and analysis. But nitrates were so mobile that when the soil was wet, they might rise to the surface when the plants were suffering from a nitrogen shortage. In moderately heavy soils subjected to drainage, it was possible to



store up nitrates in the subsoil. Nitrates could be washed down slowly and accumulated in the clods and structural units, whilst surplus water from heavy rains could get away without extracting most of the stored nitrate.

In all discussions on green manures and crop residues, it was wiser to concentrate attention on the nature of the added material, whether highly nitrogenous or not, and the time interval and weather conditions between incorporation and utilisation, instead of trying to interpret the results in terms of the hypothetical humic materials formed in the soil.

The paper concluded with a suggested scheme of two-year rotation experiments to test leguminous and non-leguminous crops and fallow as preparations for cotton, with additional tests on farmyard manure (or compost) and inorganic nitrogen and, also, on different intensities of soil cultivation.

#### DISCUSSION.

SIR JOHN RUSSELL said that Dr. Keen's paper opened up the important problem of how it was possible to ascertain whether certain soils were similar to soils already known to be suitable for a particular crop. When cultivation was expanding, it was obviously desirable to expand on to the most suitable soil, and Dr. Keen's paper suggested tests that could be applied to determine whether new soil was similar to that on which cotton, for example, was already doing well.

Dr. Crowther's paper concerned the important question of whether a system of cultivation was to be permanent or merely devised to last for a generation. In the older settled countries, where a cultivation had been evolved which was designed to be permanent, a rotation was involved that put organic matter back into the land. In the newer countries this had not, for the most part, been attempted. So long as soil was left in a state of nature, the soil was recuperated by the organic matter that fell back in the form of dead stems, dead leaves and dead roots. When, however, cultivation was begun, especially with a crop such as cotton which did not return much in the way of organic matter to the soil, an essential constituent was being removed. For a few years little difference in fertility might be observed, but in course of time the changes that occurred could not fail to be detrimental to the productivity of the crop. The question, therefore, arose as to how organic matter could best be returned to the soil under cultivation with cotton.

Dr. Crowther pointed out in his paper that rotations for cotton had not yet been evolved. Such experiments would not be easy, nor would they give any immediate return. The problem, however, had to be faced, and Dr. Crowther suggested in his paper various

means by which the organic matter could be returned, the effects which organic matter had, and consequently the reasons for returning it. It did seem very desirable that wherever experiments were being carried out, an effort should be made to include this problem within their scope. It would be interesting to hear the views of anyone who had had experience of work of this kind, or who had ideas on the subject.

MR. BAILEY said he thought that all the research workers in the Sudan Gezira would be in cordial agreement with Dr. Crowther's suggestion that particular attention should be paid to the introduction of organic matter into the soil.

The difficulty there was that, owing to the small head of stock carried, farmyard manure was almost non-existent. Composts were a possible substitute, but weed growth was, on the whole, scanty, and it was difficult to visualise the growing of crops for that purpose.

As regards Dr. Crowther's concrete suggestion of a rotation experiment, embodying cross treatments of manuring and cultivation, they already had at the Gezira Research Farm a number of rotation experiments of more or less long standing. It would not appear to be advisable to introduce cross treatments of organic manuring until the difficulties in the way of supply of that material had been overcome.

Cultivation treatments could be introduced, but their usefulness in the Gezira appeared to be doubtful, in view of the fact that experiments already carried out on the effect of cultivation before cotton in that area had indicated that such cultivation had no effect on yield. Mr. Bailey suggested that workers in other countries should make similar preliminary tests before committing themselves to the inclusion of cultivation treatments in complex experiments.

Referring to rotation experiments in general, Mr. Bailey drew attention to the necessity for paying special attention to the question of fallows. If, as in the Gezira, these fallows were normally not bare fallows, then the effect of the fallow would vary very much according to the amount of weed growth in different years. Standardization between seasons should be attempted.

MR. HOSKING said that when the Serere Experimental Station in Uganda was first opened in 1922, it was put down to a rotation which included green manuring, but it gradually became apparent that the Serere plantation as a plantation would cease to exist unless something were done, but by then it was too late. The result was astonishing, because apparently it had been farmed under what would be considered fairly good farming conditions, and yet, the season before last, 40 acres of cotton on that plantation, under European supervision, gave a lower yield per acre than the whole of the Teso district grown by natives.

*The District Agricultural Officers in the course of their duties found that the native method of agriculture, which had been regarded as chaotic and haphazard, was in fact nothing of the sort. The native practised a very definite rotation. His rotations varied from place to place, but he nearly always knew where he was in his rotation. He practised a bush fallow of periods varying from four to seven years, and this idea was being embodied in rotations which were being started at Serere.*

*At another station in Uganda, opened roughly at the same time, land had to be abandoned for the same reason, namely, loss of soil fertility; so there again bush fallowing was going to be practised. Mr. Hosking explained that by bush fallow, he did not mean quite the native bush fallow: the native left his land, and grasses and weeds came in, and after that the thorn trees. They intended to plant their grasses, and they hoped to cut out the thorn trees, because they were rather troublesome when the land was cultivated again in six years' time.*

*On the main seed farm at Serere, it was intended to have 1½ years' cropping and 4½ years' fallow. On the experimental farm itself, where variety trials of different crops and cotton were carried out, that rotation had been slightly modified, because there were a large number of working cattle on the Serere plantation, and the use of the farmyard manure would enable the period of bush fallow to be shortened. So far green manures had failed to do any good to their soils.*

*At Bukalasa an experiment had been laid down for comparing these long fallow rotations with the ordinary methods of green manuring, on a proper statistical basis.*

*Mr. Hosking mentioned a normal rotation experiment which had been started in 1929 at Serere. The first year reasonable results were obtained; the second year the yields were not very good, and now, only five years later, the cotton was the worst on the farm, and always had most blackarm infection. He thought that what he had said showed that it was unwise to follow European practice too slavishly, as regards rotations.*

*Mr. Bailey had suggested that ploughing did not always pay, and one experiment at Serere confirmed this. Certain plots were sub-soiled, and there was a significantly decreased yield of maize on those plots, and he thought that before they went any further in Uganda, they should ascertain whether the ploughing did or did not pay. It was not quite a simple matter to decide, because with a plough a native could open up more land and grow a larger area, but on the other hand he might be losing an undue amount from his soil by doing so.*

DR. WILLIS, referring to Dr. Keen's paper, said that in Brazil it had been noted that wherever a certain species of *Apeiba* grew, the soil was good for coffee, and that plant was taken as an indication of good coffee soil. As regards the return of organic matter to the soil, an article having a bearing on this and similar subjects would appear in the next issue of the *Empire Cotton Growing Review*.

MR. SENNITT said that on the Zambesi, in Portuguese East Africa, no system of rotation was included in native agriculture. Cotton was grown every year on the same soil. He had tried to introduce maize as a rotation, but it had not met with a very ready response. After cotton had been grown on the same soil for some time, the yields were very poor, but even so, the natives appeared to think that it still paid them to grow it. When the land reached a stage at which the yields were too small, they merely moved to a new piece. The problem that he had to face at his experimental farm was whether he should limit himself to the agricultural practice of the country and exhaust his land, or introduce rotations which would not be followed by the local cultivator.

Speaking of rotation experiments with cotton, Mr. Sennitt said that many years ago Dr. Prescott had started a very detailed experiment of this kind in Egypt. The experimental area was divided into two sections, one for treatment with chemical manures and the other with farmyard manure. In each section they had rotations of (i) cotton only, (ii) cotton every two years, and (iii) cotton every three years. The results had not been published, but he would endeavour to obtain them from the technical section of the Royal Agricultural Society, Cairo, if Dr. Crowther thought that they would be of use to him.

MR. LEWIN said that in Northern Rhodesia it had been found that cotton had a remarkably beneficial effect on maize grown on the same land in the following year. It produced an increase in the yields of maize of as much as 25 per cent. This was quite as big an increase as could be obtained from green manuring in the same conditions. A partial explanation of this might be that suggested by Dr. Crowther, namely that deep rooted plants such as cotton brought up nitrates which would otherwise be untapped. He suggested also that deep rooted plants such as cotton opened up the soil, which was otherwise liable to become compact. Probably both causes operated.

Unfortunately they now had no chemist in Northern Rhodesia, and much work that needed to be done on problems of this kind could not be undertaken. Apart altogether from cotton-growing, such work was needed to enable them to devise methods of maintaining the fertility of the soil generally. As he had indicated in speaking on another paper, they were about to grow cotton on a new type of soil in Northern Rhodesia, in areas where it was hoped they would be more or less free from stainers. It was impossible to foretell



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*whether the effect of cotton in improving the yield of the succeeding maize crop would apply to these new soils also, or whether cotton there would, in those conditions, have to be regarded as an exhausting crop as it was in most other parts of the world.*

*Mr. Lewin endorsed what Mr. Hosking had said with regard to native agriculture. He thought Europeans made a great mistake if they assumed at once that native agricultural methods were at fault. He thought that they were more likely to devise successful agricultural methods for a country if they made the assumption that the native was very probably right, and devoted their energies to trying to ascertain why he was right. He also agreed with Mr. Hosking as to the very doubtful advantages of the use of the plough.*

**MR. VOELCKER** said that they had been more fortunate with the practice of green maturing in Southern Nigeria than had apparently been the case in Uganda. It there appeared to be a perfectly feasible agricultural operation, if carried out over a sufficiently long period.

*On the matter of returning humus to the soil, he mentioned that they had been carrying out at Ibadan for the last seven years experiments with maize on land on which a green manure crop was also grown and burnt. No humus had thus been returned to the soil for the past seven years, yet the standard of fertility still seemed to be comparatively high. In another experiment they had grown cotton and a green manure crop year after year. The green manure was dug in about June, and the cotton planted a month or six weeks later. So far as it was legitimate to draw conclusions as to the fertility of the soil from the yields of cotton, it could be said that the fertility had been maintained, while the cotton had not suffered from any severe disease.*

**MR. PARNELL** said that in a dry climate such as South Africa, one point to be considered in connection with rotation experiments was the question of the cumulative effect of different rotation crops on the soil moisture content. They had used as rotations Tepary beans, Soya beans, maize, sunflower, fallow and cotton. Soya beans had a much more drying effect on the soil than maize, even down to a depth of five feet, and this was of extreme importance in their conditions. Moreover, it was found that the fallow was of little help in storing up moisture from one season to another. It did have that effect to some extent, but at the same time it reduced the growth of the cotton crop in the following season, particularly in the early stages. The nitrogen in the soil after the fallow was definitely higher than it was after a previous crop had been grown on the same land, but it was nevertheless noted that the young cotton crop, starting on a soil definitely high in nitrate content, grew very poorly as compared with its growth on soil on which almost any of the rotation crops had been grown in the previous season, which was, however, definitely lower in nitrate content. The fallow was



a bare fallow, and weeds were removed from it as they would be if the land were under crop. So far they had found no explanation for this phenomenon.

MR. ALLAN, speaking on Dr. Crowther's paper, said that the question of the condition of the soil and the influence of humus was of great importance. The intensity of insect pest attack and of disease was frequently intensified by poor conditions or by the physical state of the soil. He had frequently found that new varieties of a plant which under experimental conditions gave increases of yield of from 10 to 15 per cent., when distributed to cultivators gave little more than 5 to 8 per cent. increase. It was an undoubted fact that by encouraging composting, by green manuring, by slightly deeper tillage, and by various improvements of that kind in cultural methods, the cultivators were often able to get a better return from their original seed than they could from improved seed grown under the poor conditions to which they were accustomed. He urged the great importance in tropical agriculture of a close study of the conditions in which the plant was grown. He believed that further work in this direction would be most effective.

As regards the use of the plough, he had experimented for about 25 years, starting with a great enthusiasm for the inversion plough as a means of increasing production, and ending with the conclusion that the cultivator was doing very much better when he only scraped the soil to a depth of two or three inches. This was particularly noticeable with crops such as maize and cotton. With cold-weather crops in India where water had to be collected during the monsoon, slightly deeper cultivation, not by an inversion plough, but by an Indian plough working to a somewhat greater depth than usual, was more effective than the purely superficial cultivation commonly given on the black soils in India.

He confirmed other speakers who had expressed their belief in the great importance of studying closely local methods of cultivation.

MR. TURNER said they had been making an intensive study of manurial problems in Trinidad by means of field experiments during the last ten years, with special reference to sugar cane, and, though the best soil treatment for sugar cane probably differed fundamentally from that for cotton, he thought that some of their findings might be of interest to cotton workers.

He thought that without very heavy expenditure it was impossible to maintain the organic matter content of tropical soils at anything in the nature of a high level, especially under conditions of intensive cultivation. The rate of loss of organic matter under these conditions was extremely high. The effect of organic matter on the physical properties of sugar cane soils had been found to be very temporary, and in the long run very small. For sugar cane the main value of organic matter lay in its fertilising constituents—nitrogen, potash,

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and phosphate, for example. The nitrogen in the organic matter was not very rapidly available; only about one-third of the total nitrogen appeared from their experiments to be available to the crop in the first eighteen months, and only one half over four or five years. The other half seemed to be lost altogether, possibly in gaseous form.

They had also come to the conclusion that for sugar cane the organic manures which they used were not balanced, and invariably needed to be supplemented by additional nitrogen, and in certain cases by additional potash. That statement could be put in another way. Organic manures did not maintain the fertility of the Trinidad sugar cane soils with respect to nitrogen or to potash.

They had shown that on sugar cane soils it was possible completely to mask the effect of organic manures on yield by the addition of inorganic manures. For example, in an experiment with four treatments—(a) no manure, (b) organic manure alone, (c) balanced inorganic manure alone, and (d) organic manure plus balanced inorganic manure—treatment (b) gave an increase in yield, treatment (c) gave a larger increase in yield, while treatment (d) gave no larger increase in yield than treatment (c). They were trying, through the use of inorganic manures alone, to bring back into cultivation soils which had been exhausted by continued ill-treatment.

The results quoted were based on the results of an organised series of experiments carried out over a period of years on a number of different soil types.

Sugar cane was admittedly very different from cotton. It had an extensive root system, and by increasing the yield through the use of inorganic manures, the root system was simultaneously increased, and probably the organic matter content of the soil was fairly well maintained through decomposition of the additional root system.

As regards loss of nitrate through leaching, they had found that it was uncommon for nitrate to be leached below the top foot of the soils; by the time it had reached a depth of one foot it had been absorbed by the roots of the cane plant. Only in exceptional circumstances, where very heavy dressings of nitrogenous manure had been applied in wet weather, did extensive leaching of nitrate occur below the depth of a foot.

As regards cover crops and green manures, so far as sugar cane was concerned, he believed their main value under Trinidad conditions lay in the fact that they reduced weeding costs.

Replying to a question on rotation crops, Mr. Turner said that they grew sugar cane continuously: the best rotation crop for sugar cane was sugar cane.

*With noble canes, two ratoon crops were taken as a rule. With hardy canes ratooning was continued for a longer period.*

*The West Indian sugar cane soils were poor soils, very deficient in organic matter when judged by ordinary standards. Sugar cane was the only crop which could be grown economically on those particular soils.*

MR. JACKSON referred to Mr. Bailey's remark regarding the difficulty which would be encountered in the Sudan in finding enough raw organic material with which to make compost. He said that unfortunately he did not know the conditions in the Sudan, but he knew something of the conditions in Mesopotamia, and in the arid portions of Central India and Rajputana, which were definitely sub-tropical in character and did not grow the very lush natural vegetation found in purely tropical regions. In neither of those two regions would he expect to find any difficulty in procuring waste material worth using for compost. The amount would naturally vary with the district, the type of cultivation practised, the natural vegetation and the growth of weeds, which Mr. Bailey mentioned.

He believed he was correct in saying that the ordinary cultivator in Northern and Central India (speaking of it as a whole and not politically) normally produced 10-12 loads of farmyard manure per pair of bullocks employed on his holding. That would be considered good practice, a load being about  $\frac{1}{2}$  ton. On the Indore Experimental Farm, from the ordinary wastes available, very little of the land being irrigated, the output had been more than doubled.

He thought that figure was fairly significant. On that farm no stock was kept except the draught animals employed for ploughing. He suggested that if a careful survey of the problem were made in the Sudan and in other regions, it would be found that there was no inconsiderable amount of agricultural waste available. He understood that the cotton stalks and other residues of the cotton crop were burned, no doubt for perfectly sound entomological reasons. It might be better to burn them in another fashion—in the compost heap, rather than by fire.

He fully agreed with what Mr. Hosking had said on native methods of cultivation. He thought that every indigenous custom of long standing should be at first regarded as a right custom based on some sound reason, and search should be made for that reason.

Mr. Jackson said that the system of green manuring and of very long periods of fallow which had been mentioned seemed to him to be open to the objection that they were costly, and that in each case, though differing in degree, they placed the soil out of cultivation.

He went on to suggest that possibly those of them who had been concentrating on the cotton crop had been rather too prone to shut their eyes to its relationships with other crops. This had perhaps

led to undue importance being attached to the plant breeding aspect. Quoting his predecessor at Indore, Mr. Jackson went on to say that whereas the efforts of the plant breeder might be expected, perhaps, to raise the returns of any crop with which he was dealing in the region of 25 to 30 per cent., or even 40 per cent., it was quite possible in many parts of the world, with comparatively small effort, to raise the production, by ameliorating soil conditions and general cultivation, by at least 100 per cent. The agronomical side, therefore, merited more attention than had been given it in the past.

Very concentrated attention had been paid to the problem of composting agricultural wastes at Indore during the past five years, and he thought he could say that it was an absolutely practical proposition for any cultivator, however ignorant, however poor, and however small his resources, to increase very largely the production of organic manure from his holding. For the last four years compost had been produced, and such improvements as had been made had been mainly simplifications in technique. He could not help feeling that a good deal of the loss of organic matter which occurred in tropical and sub-tropical regions could be replaced by some such system as that which had been worked out at Indore. It might require local modifications, but so far it had not failed to deal with any local set of conditions, whether in respect of materials or of climate, with which it had had to contend.

MAJOR CAMERON said that his experience confirmed Mr. Parnell's as to the harmful effects on the succeeding cotton crop of a bare fallow for one year. They had also found in Southern Rhodesia the same effect that Mr. Lewin had recorded in respect of Northern Rhodesia, namely that cotton gave a considerable increase in the yields of maize grown on the same land in the year after the cotton crop. On the other hand, he differed from those speakers who took the view that the agricultural methods adopted by the natives were probably those best suited to the country. In the Native Reserves in Southern Rhodesia the practice of shifting cultivation had resulted in the scouring up of old lands, and the Government's Native Development Department was at present engaged in trying to inculcate better practice. They were meeting with considerable success, and the natives themselves were demanding to be shown the improved methods of agriculture.

MR. LEWIN explained that when he had said that the native practices might be assumed as being best, he had intended to qualify it by saying that they were the best under the conditions in which the natives had always lived. In Native Reserves the limitation of land introduced a new problem, and one of the tasks of the Agricultural Department was to help the native to adapt himself to the new conditions.

SIR JOHN RUSSELL, in dealing with the points raised in the discussion, said that he thought that with regard to native methods of cultivation it should be remembered that it had at all events the merit that it had been going on for a very long time, whereas it was still to be discovered



*whether the modern methods of cultivation which had been introduced into tropical countries would prove permanent.*

*Several of the speakers had referred to the disadvantage of deep ploughing. Probably in hot, dry conditions shallow cultivation was better, but in wet conditions the advantage lay with deep cultivation ; but, like all generalisations, that was liable to very many exceptions.*

*Referring to the continuous cultivation of cotton on the same land, Sir John Russell said that any non-leguminous crop could be cultivated continuously, so long as one was not very exacting in the matter of yield. He instanced the fields at Rothamsted, where wheat had been grown on the same land for over 90 years, barley for over 80 years, and mangolds for about 60 years. Those crops were still growing satisfactorily, though the levels of yield were not so high as those obtainable in a rotation. One of the objects in studying rotations was to endeavour to arrange a sequence in which one of the crops would give a better yield than might otherwise be expected, as apparently happened in Rhodesia when maize followed cotton.*

*He had been interested in Mr. Turner's remarks on the effect of organic manures on sugar cane. Organic manures were not quite the same thing as the supply of organic matter to the soil. They did afford one way of returning organic matter to the soil, but it was usually an expensive way. In England the only organic manure which would be regarded as effective in increasing the organic matter in the soil was farmyard manure, which was not generally easily available where cotton was grown, and one had consequently to find some other method of obtaining organic matter. He was very interested to hear from Mr. Jackson something about the Indore composting process which Dr. Crowther had mentioned in his paper.*

**DR. KEEN** said he had been impressed by the critical attitude of several speakers towards cultivations. Up to about twenty years ago, cultivations were regarded as something which had to be done in any case, and done well. Studies at Rothamsted of cultivation problems had convinced him that a considerable amount of the time and money which was put into cultivations in this country was unnecessary. For example, the extra surface cultivations which the good farmer gave to his root crops did not necessarily increase the yield ; in fact the yield might even be reduced. They had had that experience at Rothamsted on different types of soil and with different crops.

*There was also the question of deep versus shallow cultivation. Everything depended on the particular type of soil which was being dealt with. At Rothamsted deep cultivation could not be done suddenly, because of the clay subsoil being so near the surface, but it could be done and had been tried, but no benefit worth the expense had ever been obtained : the improvement was only an extra half ton of potatoes on land which did not give a very high potato yield, and there was no benefit to the succeeding crop.*



Referring to Indian conditions, Dr. Keen agreed almost entirely with Mr. Allan. Many of the efforts made with a view to improving cultivation in the conditions there had been just so much waste, but he recalled the excellent crops that could be obtained with the most modern systems of deep cultivation, employing tractors and cable implements, on the typical Gangetic alluvium, where the soil was deep and permitted of that extra-vigorous treatment. That statement might serve as a summing up of the views put forward by a number of speakers, that the indigenous cultivation methods found in a country were probably correct. They were almost certainly correct within the limitation of the tools that the native employed. In the case of the Gangetic alluvium, for example, where native cultivation was done to a relatively shallow depth, one could not argue off-hand that that was the best form of cultivation, for the simple reason that deep cultivation was beyond the resources of the native cultivator.

The crux of the whole thing was that the function of cultivation had been rather misunderstood in the past. Its main function was to produce the right type of tilth for the seedling and growing plant. A very important subsidiary function, as Mr. Bailey had remarked, was to keep down the competition of weeds for moisture and nutriment. These two requirements, however, covered practically everything that cultivation had to do. There was no special virtue in cultivation *qua* cultivation. The conception that a mulch produced by cultivation would conserve moisture was probably incorrect. Its main effect was to reduce the competition of weeds with the crop; in so far as its direct physical effect was concerned, views had altered very considerably during the past few years.

DR. CROWTHER said that his paper had fulfilled its purpose of initiating a discussion. Several speakers had shown how simple traditional European methods often broke down when transplanted to native conditions. Simple rotations would probably be found to fail in this country too, if certain vital factors in the traditional methods were neglected. Thus, simple green manuring failed to maintain fertility at Woburn. Many of the teachings on crop rotation were convenient rationalisations from practices which depended on ensuring efficient sheep-keeping on light land. If one left out all the financial considerations associated with the sheep, it was scarcely possible to discuss the effects of the individual crops on soil fertility, for they had not yet been tested.

Dr. Keen had dealt with some of the cultivation questions raised. An essential point of his own paper, Dr. Crowther said, was that one must regard the cultivation of the soil by implements, by plant roots, and by the weather as the most important factors in all consideration of crop rotation.

Finally, he wished to support very strongly Mr. Jackson's appeal for a fuller study of the agronomic side of the work of the cotton experiment stations.

## CONCLUSION OF THE CONFERENCE.

*Sir James Currie* said that on behalf of the Chairman of the Executive Committee, Sir Richard Jackson, he was asked to apologise that he had had to return on business to Manchester. It was a great pleasure to the Corporation to welcome those who had attended the Conference, and the Corporation extended their best thanks to all those who had expended so much time in compiling the papers that had been submitted and to all who had attended, and especially to those who had taken part in the discussions. He was particularly glad to see so large a number of younger members present, and he extended a warm welcome to all. He trusted that circumstances might permit a third Conference on similar problems to be held in another four or five years' time.

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